SCIENCE

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RECENT STUDIES ON THE BIOLOGICAL EFFECTS OF RADIOACTIVITY[1]

X-RAYS were discovered in 1895 and the first of the publications which placed Madame Curie, the discoverer of radium, in the position of foremost woman of science, appeared in 1898. The application of these results to biology, a matter of great importance, was brought about through accident. A knowledge of the physical properties of radio-active substances would lead one to expect that the physiological action would be acute, and that fact was accidentally proven to be true.

Becquerel carried a small tube of an impure radium preparation in his vest pocket for six hours. A few days later he observed a reddening of the epidermis of the abdomen opposite the location of the pocket in which he had placed the radium compound. It was not long before the inflammation became pronounced, and an ulcer developed which required several months for the healing.[2]

Giesel exposed the inner portion of his arm, for two hours, to 0.27 gram of a radium preparation, enclosed with a double celluloid capsule. After two or three weeks the skin reddened, blisters formed and the epidermis peeled just as with a burn. The growth of hair was also destroyed and did not come out anew, although a smooth white skin reformed.

Madame Curie had learned very early in her studies that radiation affects tissues, for she says in her thesis, "The action of radium upon the skin can take place across metal screens, but with weakened effect."

Thus early began the application of a

[1] Contribution from the Zoological Laboratory of the University of Texas, No. 125.

[2] From Baskerville, "Radium and Radioactive Substances," Williams, Brown and Earle, Philadelphia, 1905.

method which has now an established place in therapeutics, in the treatment of skin diseases, warts, tumors, cancers, etc. An imposing list of literature from the clinical standpoint bears witness to the practical importance of radioactivity, and many contributions show the interest of medical men. Now all physicians must be familiar with the results of work in this line, and, in addition, every city boasts its experts and specialists in radiography and radiotherapy.

Thus far, however, clinicians have been chiefly concerned with the formulation of effective methods of treatment, and it has been left for the biologists rather than the physicians to analyze the effects of radioactivity upon living matter.

The attack on the problem of the effect of radioactivity on tissues and organisms and the use of radioactivity as an experimental means of studying questions of more fundamental biological importance was at first insignificant. Even yet our knowledge of the effects is very meager; and to our ignorance of the deeper-lying basic principles which govern the action of radium and of X-rays is due the uncertainty with which the extension of the methods to new fields and new problems is viewed by many medical men. The analysis of the effects has been taken up only recently and progress in the investigation has not been rapid, with the result that very little constructive work has been done.

In studying the effects of radioactivity, both radium and X-rays have been used as a means of experiment, and the literature of both may be considered together on the basis of the current working hypothesis that the effects of both are comparable, especially in the case of the gamma rays of radium. Radium rays are of three kinds, alpha, beta and gamma; of these the gamma rays are the most penetrating, and to them are probably due most of the effects on living forms. From comparative studies of the physicists it is well known that the gamma rays of radium are quite similar in many particulars to the X-rays, and it is stated by Rutherford that they are, in fact, the more penetrating X-rays. Some recent experiments seemed to indicate that the effect of the other rays is by no means negligible, for with the preparation used, when the rays were deflected by a magnet the effect of the beta rays was stronger than that of either of the others. Even in this case, however, the effects seem to differ in degree rather than in the kind of their action, and the results are not in conflict with the hypothesis which is now serving as a working basis for experiment.

In general, it may be said that when living cells are exposed to action of radioactivity, the vital functions are retarded or depressed and a permanent injury may result; this depends on three factors, the strength of the radiating substance, the duration of the exposure, and the distance of the object from the source of the radiation. When the intensity of the radiation is great, owing to exposure at short range to a strong preparation (or strong current in the case of X-rays) for a long time, the effects are much more injurious than when the intensity is less. Indeed, numerous cases have been reported where a qualitative difference results from a slight radiation as contrasted with one of great intensity, for frequently stimuli which will retard growth if of high degree, will be found to accelerate it if weak enough. Exposure to rays of great intensity has been shown to retard or stop growth, differentiation and regeneration, and to interfere with the processes of cell division, sometimes causing degenerative changes to take place in the nuclei, and in one case at least to induce amitosis where indirect cell division had normally been the method of multiplication.

In rapidly growing tissue, such exposure will cause a decrease in the rate of division as well as interfering with its regularity. On the other hand, an exposure of short duration and of slight intensity will in some cases stimulate growth, and accelerate regeneration, and may perhaps cause an increase in the rate of cell division.

The literature[3] on radioactivity and its biological effects is voluminous, but there are only a small number of papers dealing with the question from a biological point of view. There were a very few early papers on the effect on growth and cell division; they, however, were pioneers in this field and their results are not far reaching. Most of the work done at that time, and indeed the majority of all work on the problem, has sought to use radioactivity for the study and solution of questions which were purely medical. Of the early experiments those of Perthes,[4] of Schwarz[5] and of Schaper[6] have perhaps had the most bearing on the later development of the problem. These early investigations were done about 1903–05. The pioneer stage of the investigations may be considered to end with Gager, who has been the most important botanist to make contributions to this subject. In his monograph he sums up all of the work that has been done and adds many facts from his own experiments. The work of all these men is representative, and may be regarded as showing the state of progress at that time. The results of each made a distinct contribution to our knowledge of the effects of radioactivity, and are here considered in their chronological order because of their bearing on the subsequent development of the subject.

The work of Schwarz is of importance because from his experiments he erected a hypothesis, the lecithin hypothesis, to explain the destructive effects of radioactivity. His hypothesis "was based on the fact that egg yolk is decomposed by exposure to the radium radiations. Although the matter was not chemically determined, it seemed probable that the lecithin was broken up into cholin and tri-methylamine and other end products of lecithin decomposition. Lecithin has been found by many investigators in all cells, especially in egg yolk, spermatozoa, pollen cells, plant spores, growing buds, and in all rapidly growing tissue. If, then, it is destroyed, such cells must necessarily be unfavorably affected" (Packard). According to Schwarz the effect of radiation on chromosomes and other cell organs is an indirect one where it occurs at all, being brought about as a secondary result of the decomposition set up. It is especially at this point that the lecithin hypothesis comes into conflict with that of Hertwig, to be discussed later.

Perthes' observations were among the first from a biological point of view. He exposed *Ascaris* eggs to radium and noted that the cell divisions became slower than in the controls, where no exposure had been

[3] No attempt is here made to offer a comprehensive bibliography on this subject, for such lists are available in Gager, C. Stuart, "Effects of the Rays of Radium on Plants" (*Mem. N. Y. Bot. Garden*, IV., 1908); in Warthin, A. S., "An Experimental Study of the Effects of Röntgen Rays on the Blood-forming Organs with Special Reference to the Treatment of Leukæmia" (*International Clinics*, Vol. 47, 1906); in the publications of Bardeen, of the Hertwigs and others. Only references are here included which bear directly on our present theories for the interpretation of the observed effects.

[4] Perthes, G., "Versuche ueber den Einfluss der Röntgen- und Radiumstrahlen auf Zellteilung," *Deutsche Med. Wochenschr. Jahrg.*, Bd. 30, 1904.

[5] Schwarz, G., "Ueber die Wirkung den Radiumstrahlen; eine physiologische chemische Studie am Huhnerei," *Arch. f. Physiol.*, Bd. 100, 1903.

[6] Schaper, A., "Experimentelle Untersuchung ueber den Einfluss der Radiumstrahlen auf Embryonale und regenerative Vorgänge," *Anat. Anz.*, Bd. 25, 1904.

made. The controls gave rise largely to active worms, but radiated eggs developed only into irregular cell-masses or misshapen little worms which were especially irregular at the tail end, the results depending upon whether stronger or weaker intensity of radiation was used. He says that all three elements which normally take part in mitosis were formed. Centrosomes, spindles and fibers were alike all clear in the experiment and in the controls. The chromosomes divided irregularly, and in *Ascaris megalocephala univalens* the characteristic number was doubled, knotty swellings appearing on the chromatin loops instead of the normal arrangement, and in some places the chromosomes were broken up into numerous unequal pieces. The injury to the eggs expressed itself in the slowing down of the development, which gives rise to abnormal individuals. The results with X-rays were entirely analogous to those with radium, delaying cell division and giving rise to abnormal products of development. In both cases, the chief effects of radiation on the cells appeared indirectly, but only after the lapse of a certain period of time. Eggs in the resting and in the dividing condition served equally well for the experiments.

Schaper exposed frog eggs as well as *Triton* embryos to radium for varying lengths of time. He observed an inhibitory effect on cell division, on embryonic differentiation, and on growth. There was also an inhibition of regeneration which was recognized after a longer or shorter period. The latent period usually lasted almost a day, the duration depending on the intensity of the radiation and on the stage of development of the organism. The period of development was always more or less retarded and prolonged. Finally it reached a standstill and then death resulted. He thinks there is some relation between the manner of solution of the yolk and the effect of the radiation. In older larvæ, the living substances were used up and acute degenerative changes in the cells set in.

Gager[7] reviews the literature on this subject up to 1908 and summarizes the state of knowledge at that time in his last paragraph as follows:

> The broadest, and at the same time the most definite generalization warranted by the work so far done is that the rays of radium act as a stimulus to metabolism. If this stimulus ranges between minimum and optimum points, all metabolic activities, whether constructive or destructive, are accelerated, but if the stimulus increases from the optimum toward the maximum point it becomes an over-stimulus, and all metabolic activities are depressed and finally completely inhibited. Beyond a certain point of over-stimulus recovery is impossible, and death results.

His review of the previous investigations he brings together in the following statements:

> 1. Radium rays have the power to modify the life processes of both plants and animals.
>
> 2. Röntgen rays and radium rays produce similar physiological results.
>
> 3. Sensitiveness to these rays varies with the species of either plant or animal.
>
> 4. Younger, and especially embryonic tissues, are more sensitive than those more mature.
>
> 5. With only one or two exceptions, exposure to radium rays has been found to either retard or completely inhibit all cell-activities. The rays may cause irregularities in mitosis.
>
> 6. Experimental evidence for or against the existence of a radiotropic response is conflicting.
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> 7. Whatever the immediate, internal change produced in the protoplast may be, the result, with animals as well as with plants, appears to be more or less profoundly modified by the presence of chlorophyll in the cell.
>
> 8. Radium rays appear to retard the activity of enzymes.

Since the publication of Gager's results on the effect of radium on plants a large amount of work has been done in the labo-

[7] *Loc. cit.*

ratory of Oskar Hertwig[8] at Berlin. Hertwig himself, his son Günther, and his daughter Paula, and a number of his students have performed an extensive series of experiments over a wide range of forms and have obtained results which are of the greatest significance. In all of their work, radium compounds have been the source of the rays used for experimental purposes. As a strong exposure to radioactivity is always injurious to tissues, and since the development of injured eggs gives rise to malformation and produces monsters of various degrees of deformity, much of the experimental work is teratological in nature. This is interesting from a pathological standpoint, but is perhaps less fundamental than the effects of the radiation on cells (*e. g.*, egg cells) and on their activity. On both these phases of the study the work of the Hertwigs has an important bearing.

The theory which was developed by the early work of the Hertwigs and which has been the working hypothesis upon which their subsequent studies have been made is called by its author a "biological hypothesis." The observation was made in the first cytological studies that centrosomes, spindles and other cell organs with the single exception of the chromatin showed little injury due to the action of the rays. This conclusion was based on evidence from the study of eggs and sperm of sea urchins and of frogs; later the observations have been extended to other forms. It led to the assumption that the effect is a direct one on the chromatin of the radiated cells, not an indirect one as had been postulated by Schwarz, and, further, that the seat of the injury if not exclusively in the chromatin is certainly chiefly there. Due to the fact that a slight radiation of the sperm is sufficient to cause abnormalities in the embryo, it was held that the injured chromatin possesses the property of conveying the injury to the egg cell when it fuses with it and subsequently to the descendants of this cell, for nuclear division provides the mechanism for distributing the injury to all cells of the body. In a sense, therefore, the original injury tends to increase as development proceeds. Hertwig sees in the beta and gamma rays of radium a reagent which affects the nuclear substance of living cells even in the slightest amount. Especially the chromatin is injured in its living properties by the slightest exposure to radio-active rays, and by a greater exposure is so changed that it loses the capacity to grow and to increase in the regular way by mitosis, and undergoes a gradual degeneration into which the cytoplasm is also drawn.

It may be said that this hypothesis has much morphological basis and that it is sufficiently elastic to accord with many of the observed facts; yet it is clear that no real explanation of the phenomena has been offered on this basis, for the problem is simply pushed further back into the cell and it is necessary to make clear how the chromatin is injured and how the injury accumulates with development. It is undoubtedly true also that other substances in the cell than chromatin are injured, although it may not be possible to attribute the irregularities of later development to them, as can be done in the case of the chromatin. A comparison of this hypothesis with the lecithin hypothesis, and criticisms which have been made of each, may be deferred until other facts have been brought out.

Recently Oskar Hertwig[9] has brought together in a brief statement the facts most

[8] A series of papers by O., G., and P. Hertwig, by Oppermann, Fraenkel, and Stachowitz in the *Arch. f. Mikr. Anat.*, Bd. 77 to Bd. 85, 1911–1914.

[9] Hertwig, O., "Versuche an Tritoneiern ueber die Einwirkung bestrahlter Samenfäden auf die tierische Entwicklung," *Arch. f. Mikr. Anat.*, Bd. 82, Abt. II., 1913.

important from his interpretation which have been obtained as the results of the investigations in his laboratory. The following facts are emphasized by him:

1. Fertilized *Ascaris* eggs, which had been radiated several hours showed pathological nuclear division figures in which the chromatin bodies are represented by irregular masses of chromatin granules. They divide slowly and begin at last to degenerate by caryolysis. (Paul Hertwig.)

2. By intensive radiation of several hours' duration, sperm threads of the sea-urchin are so affected that, while they are able to fertilize the egg and to stimulate the egg nucleus into spindle formation, they lose their ability to form normal chromosomes and thus are eliminated from development partially or completely, sooner or later, depending on the degree of the injury. (Günther Hertwig.)

3. An elimination of the sperm nucleus which is derived from intensively radiated sperm threads has been observed during the first and second divisions in eggs of the frog (Paula Hertwig) and of the trout (Oppermann).

4. For the elimination from the development process of the radiated sperm chromatin which has lost its capacity to develop, the fact established beyond doubt for the radiated larvæ of *Triton* speaks convincingly, that the somatic nuclei have only half, or the reduced, number of chromosomes. Since the male chromosome complex fails to take its part in the development, due to the radiation, the somatic nuclei have only the female complex. (Oskar Hertwig.)

5. This fact agrees with the result obtained for frog, toad, *Triton* and trout embryos, that after the maximum radiation of the sperm which are to be used for fertilization, the nuclei of the different cells are strikingly smaller than are those of the controls of the same age, and both their surfaces and their volumes are in ratio to those of the control as 1:2. For the conclusion, that the chromosome number of the nucleus is the haploid, may be drawn from the fact established by numerous experiments that the volumes are reduced almost to half. (Oskar Hertwig, Günther Hertwig, Oppermann.)

6. The results obtained from cytological investigation offer the possibility of explaining a fact highly remarkable from a physiological standpoint and at first glance a very puzzling one, that eggs, which would ordinarily fail to develop from the germinal vesicle stage when fertilized by foreign sperm because of the union between disharmonious idioplasms, are spared from destruction and may develop into larvæ if only the sperm from the different species are radiated intensively before fertilization. The puzzle is solved by the simple reflection that the effect of the union of the disharmonious idioplasms with its disastrous consequences is avoided by the injury to the radiated chromatin, although the sperm thread penetrates into the egg and stimulates development. Although the radiation of the strange sperm has been destructive to the sperm, it has been favorable to the fertilized egg, just as in the living body a poison substance is counteracted by another poison.

The investigation of radiation effects on *Ascaris* eggs was undertaken by Fraülein Paula Hertwig in order to obtain definite evidence on the facts at the basis of the biological hypothesis. Her conclusion is that the division of the eggs is retarded, and pathological appearances are very soon noticeable after the radiation; chromatin is strongly affected, as already stated, although the centrosomes, spindles and other cell organs show no injury; unfavorable action of the cytoplasm is not to be assumed since no change can be seen. She is able to

find no ground for the lecithin theory, but interprets her facts from the contrary viewpoint. The effect of radium upon *Ascaris* eggs has been reinvestigated by Payne[10] who confirms the results just given.

Günther Hertwig, following the lead of his father, in his various studies set up four series of experiments, the A, B, C and D series. In the A series, eggs were radiated in the two-cell stage, or after fertilization; in the B series, sperm were radiated and then used to fertilize normal eggs; in the C series, eggs were radiated and then fertilized by normal sperm; in the D series, both eggs and sperm were radiated before fertilization. The experiments described in his first paper were on the frog's egg. He found that in the C series (normal sperm by radiated eggs) the injury increases with the duration of the radiation up to a maximum, and from there on decreases again as the radiation is prolonged. Only the radiated nuclei show the effects, and there is no evidence for the hypothesis that yolk which contains lecithin is being broken down. The injury is greatest on young tissue and on tissues which are to be highly specialized; it is productive of manifold disturbances of the developmental processes. In the frog development is possible with only a haploid nucleus, that is, only the half of the nucleus derived from one parent. Where the injury is severe to either the egg or sperm nucleus, the other is able to carry on the development, and in fact there is less interference with the regular course than in the case where both nuclei are injured slightly and both take part in the process, for here the injured half is no longer a factor; it is able only to stimulate development and then is eliminated from the process. In other words from the standpoint of heredity, this is a case of parthenogenesis. For the spermatozoon always has two functions to perform: it must initiate development, and it must carry the inheritance from the male line. In this case only the first function is accomplished, and parthenogenesis is the virtual result, the male pronucleus being eliminated from development by failing to unite with the female pronucleus. This result is important as evidence that the nucleus is the bearer of the inheritance substance and that in the beginning of development male and female nuclei are of equal significance.

Subsequently Günther Hertwig performed a similar set of experiments on the eggs of the sea urchin. Here he was able to work out the cytological details of the process and to establish firmly his view just stated, that the eggs after intense radiation really undergo parthenogenetic development. This is the most important point of his contribution, although he presents much evidence for the biological hypothesis.

A great many other experiments have been carried on in Hertwig's laboratory at Berlin, and all of them contribute to the same conclusions. The principles already discussed are the most important ones brought out, and they are supported by a large amount of evidence; many data also have been gathered from these experiments which are valuable from the standpoint of teratology. This, of course, is incidental to the evidence for the biological hypothesis.

In America, although much attention has been given to the medical aspects of radioactivity, very little work on the biological phases of the problem has been attempted. Bardeen[11] has carried out systematic ex-

[10] Payne, F., "A Study of the Effects of Radium upon the Eggs of *Ascaris megalocephala univalens*," *Roux Archiv.*, XXXVI., 1913.

[11] Bardeen, C. R., "Abnormal Development of Toad Ova Fertilized by Spermatozoa Exposed to Röntgen Rays," *Jour. Exp. Zool.*, IV., 1908. "Variations in Susceptibility of Amphibian Ova to the X-rays at Different Stages of Development," *Anat. Record*, III., 1909. "Further Stud-

periments of the toad's and frog's eggs, demonstrating that abnormal embryos result from radiation, and showing to a certain extent under what conditions they are produced. The kinds of abnormalities are described and experiments given to determine the periods of greatest susceptibility. The following are among his most important conclusions:

> These experiments show conclusively that both the male and the female sex-cells may be so altered by the X-rays as to give rise to the formation of monstrous forms. The susceptibility of the male and female sex-cells is approximately equal, although the abnormalities appear earlier in development and are greater when the ova are exposed. After fertilization until cleavage begins, the ova at first appear to be no more susceptible than the sex-cells before fertilization. During the earlier stages of cleavage the susceptibility of the eggs to the X-rays is markedly increased, but during the later stages of cleavage before closure of the blastopore the susceptibility of the eggs becomes much less, and after the blastopore is closed the power of the X-rays to influence development becomes strikingly reduced. The period of greatest susceptibility is the period during which there is the most rapid production of nuclear material.

Packard[12] has recently published an account of his experiments on the effect of radium on the fertilization of *Nereis*. These experiments were performed to ascertain how "early the development of the egg is affected by radium radiations when (1) the sperm is exposed; (2) when the egg is exposed; and (3) when the egg is exposed immediately after fertilization." His results are important, for he finds that, in addition to the usual effects such as retardation of development, multipolar spindles and the like, not only chromatin, but also the achromatic portions of the spindles and the cytoplasm, show the effects of the exposure. In eggs radiated before fertilization, it may happen that the alveolar layer of the cytoplasm is not extruded as is normally the case in the eggs of *Nereis*. If this occurs the maturation processes are much modified, resulting in diverse forms of chromosomes and spindles, with perhaps small asters scattered about through the cytoplasm; and various other irregularities may be present in the mitotic figures. Thus it will be seen that Packard's observations do not all agree with those made in Hertwig's laboratory. Parthenogenesis is not found to occur in *Nereis* as the result of exposure to radium, but it is a common observation that the eggs of that animal are not as favorable for parthenogenetic development as are those of the sea urchin upon which Hertwig worked.

For various reasons the hypotheses of both Hertwig and Schwarz are held to be insufficient to account for the phenomena observed by Packard, and he proposes another explanation, suggesting "that the radium radiations act indirectly on the chromatin and protoplasm by activating autolytic enzymes which bring about a degeneration of the complex proteids, and probably by affecting other protoplasmic processes in the same manner." This hypothesis is reached partly as a result of his own experiments and partly from a consideration of certain other work, and to some extent takes a middle ground between the other two, although in some phases it differs sharply from both. Cells contain a great many kinds of enzymes and it has been shown by a number of investigators that radium rays and X-rays have the property of modifying the action of some enzymes. Packard concludes that while many enzymes may be activated, "the lytic enzymes are more stimulated than those which play a synthesizing rôle." Where a

ies on the Variation in Susceptibility of Amphibian Ova to the X-rays at Different Stages of Development," *Amer. Jour. Anat.*, Vol. 11, 1911.

[12] Packard, Charles, "The Effect of Radium Radiations on the Fertilization of *Nereis*," *Jour. Exp. Zool.*, 16, 1914.

slight radiation results in acceleration, the synthetic processes may be supposed to be stimulated more than the destructive activities. This hypothesis is essentially chemical in nature and seeks to explain the morphological effects observed in the cells as the indirect result of enzymotic activity under the influence of the radiation.

Various new arguments for and against these hypotheses and theories have been brought out as new facts have developed. It is, therefore, necessary to consider the more important points critically.

The lecithin hypothesis was established on the basis of an experiment by Schwarz, showing that in the chicken egg yolk, containing lecithin, is broken down under the influence of radium. Against this hypothesis numerous objections are now to be raised. Hertwig pointed out that the decomposition was not determined by strict chemical methods, and Neuberg also criticizes it on chemical grounds, for lecithin is itself so unstable that only a very accurate chemical study could determine whether its decomposition was actually due to the radium radiation.

If the seat of the injury were in the yolk, little effect could follow radiation of the sperm before fertilization, for the sperm at most contains but little lecithin, and contrarily, much greater injury should result when the egg is radiated. As a matter of fact, there is very little difference between the results on the embryo, whether it is the egg or the sperm that is radiated before the fertilization. Egg nuclei are equally capable, with those of the sperm, of contributing the chromatin by which the parthenogenetic development previously described takes place, both at first are able to start the development of the egg so far as the hereditary units are concerned, and injuries arising from the disturbance of either are equally great.

Very little lecithin could be decomposed when so short a radiation of the sperm as a minute was employed, as by G. Hertwig; yet this radiation was sufficient to cause marked departures from the normal in the embryo. That no chemical poison is generated by lecithin decomposition is obvious, for in the case of the short radiation of the sperm the poison would be too dilute to cause effects equal to those resulting from longer radiation of the egg.

The lecithin theory takes no consideration of the fact that the most careful cytological study shows no morphological evidence of yolk destruction; nor of the fact that nuclei and especially chromatin do suffer marked changes as the results of the radiation. On the latter point all investigators agree, and it must be explained by any hypothesis which seeks to account for the changes produced. Furthermore, yet other cell constituents are acted on by radiation, for cell extracts and cell enzymes have been shown to be activated or retarded in their action, depending upon the conditions of the experiment.

In this connection, it is by no means clear how lecithin decomposition within the egg could prevent, as Packard found radiation to do, the extrusion of the alveolar layer of the cytoplasm, a phenomenon depending upon activities at the surface of the egg. And finally, it is not possible to account on this basis for the elimination of the sperm head from development, or for the fact that somatic nuclei of radiated *Triton* embryos contain only half or the reduced number of chromosomes.

Most of these arguments against the lecithin hypothesis were, of course, unknown at the time it was proposed. They are so much at variance with it, however, that it seems impossible to give it further serious consideration. It may be regarded as completely overthrown.

Similar explanations have been suggested by others, *e. g.*, by Hippel and Pagenstrecher[13] who find that *x*-radiation of rabbits produces an effect similar to that resulting from cholin injection and think that a toxin is developed which is transferred from mother to embryo, to the injury of the latter. The facts already brought out here show that this explanation is not a sufficient one, nor one of general application.

The objections to Hertwig's biological hypothesis are less serious than in the case just discussed, and they would scarcely hold against it in a less extreme form. It is, of course, true that the solution offered by Hertwig is incomplete in that it does not go far enough back into the organization of the cell; for, even on the assumption that the chromatin is the chief seat of injury, further explanation, which must ultimately be chemical in nature, is required to show how the injury is communicated to other parts of the cell and what the mechanism is by which its action is manifested.

According to the Hertwig hypothesis, chromatin above everything else in the cell suffers injury from radiation and the pathological conditions in the embryo are traceable to the injured chromatin, which may be regarded as a "contagium vivo" increasing, ferment-like, at each division.[14] Thus the mechanism of the cell provides the means for distributing the injury to each successive cell generation and for carrying it to all parts of the embryo.

To this theory, Packard has offered two criticisms. The concept, that the injured chromatin or a substance produced from it acts as a contagium vivo, is scarcely a solution of the problem, for it merely restates in another form certain facts observed and presents a picture of the problem itself from a different viewpoint without giving any explanation of the facts. It should again be pointed out that the ultimate solution must be chemical in nature. Packard also questions the assumption that the injurious substances developed in the nucleus must remain there and can not involve purely cytoplasmic structures even during division (for a normal haploid division of the egg chromosomes takes place if the radiated sperm head does not mechanically interfere in the spindle). A condition contrary to this assumption is given by Packard, showing that cytoplasmic structures are changed in *Nereis* eggs, for example in the case of eggs radiated before fertilization which fail to give off the alveolar layer and thus extrude the jelly as they should do. Obviously here the injury has been communicated to the egg cytoplasm, and is not in accord with the Hertwig assumption.

But the most serious objection to the Hertwig theory as it now stands lies in the fact that other than nuclear structures and substances are affected by X-rays and the radium rays. The failure to extrude the alveolar layer in *Nereis* eggs is a case in point, and in the same eggs abnormal spindles and asters occur as a consequence of radiation. It has been found by numerous investigators that radium rays have the power to affect enzymes, and the writer[15] has shown that X-rays are able to bring about modification in the activity of certain enzymes. Enzymes are derived from living tissues, and if it is possible to cause their modification outside of the cell by the use of radioactivity it is not improbable that they also undergo change while within the cells. In fact, the writer working with Miss Woodward[16] was able to prove that X-rays

[13] *Münchner Med. Wochenschr.*, No. 10, 1907.

[14] Hertwig, O., "Die Radiumkrankheit tierischer Keimzellen," *Arch. f. Mikr. Anat.*, Bd. 77.

[15] Richards, A., "The Effect of X-rays on the Action of Certain Enzymes," *Amer. Jour. Physiol.*, Vol. 35, 1914.

[16] Richards, A., and Woodward, A. E., "Note on the Effect of X-radiation on Fertilizin," *Biol. Bull.*, V., 28, 1915.

can be used to influence the activity of the cell extractive called fertilizin. This substance is extracted from sea-urchin and starfish eggs when the ripe eggs are allowed to stand in water for a short time, and it possesses the property of causing the agglutination of the sperm of its own species. Its behavior is in some respects comparable to that of an enzyme and it is possible that the substance contains enzymotic bodies. The experiments showed that radiation by X-rays is capable of changing the activity of fertilizin, and in general agrees with previous work that weak radiation is accelerative and strong inhibitive. Fertilizin is a substance derived from the living eggs and the extraction takes place while the egg is in the resting stage, sometimes even in the germinal vesicle stage; at this stage chromatin can scarcely play any part in the giving off of fertilizin. In this case, then, the radiation has had a considerable, and a measurable, effect on a cell substance independent of the chromatin or other nuclear structures. This fact can hardly be brought in line with the Hertwig hypothesis in its present form.

Yet it is true that chromatin and nuclear structures are greatly changed by radiation. In any true explanation that may be given this important fact must be dealt with. It is possible that a modification of the present form of the Hertwig theory in which the effect on enzymes is recognized may be sufficient to account for all the facts that are now known.

Packard has attempted such a modification in his suggestion "that the radium radiations act indirectly on the chromatin and protoplasm by activating autolytic enzymes which bring about a degeneration of the complex proteids, and probably by affecting other protoplasmic substances in the same manner." Against this hypothesis there is little that can be urged except the fact that it rests upon insecure evidence, there being but few actual observations or experiments which contribute to it. It is certain that radiation influences the activity of various enzymes, but there is very little evidence upon which to base the assumption necessary to the hypothesis that those enzymes which cause katabolic changes in the cell proteids are accelerated to a greater extent than those which have the opposite function. For this reason judgment can only be suspended until such a time shall come when accurate and more abundant data are at hand for attacking the problem.

Joly[17] has proposed a different kind of explanation to account for *x*-ray effects. Comparing the events which take place in a photographic film with those which occur in cells subjected to gamma or to *x*-rays, he supposes that the rays increase ionization in the tissue. The various results found are accounted for as due to differing degrees of ionization and to the presence or absence of an "intensifier" or an "inhibitor." No evidence from biological studies is given to support this hypothesis, however; it must, therefore, await experimental confirmation.

It will be seen from the foregoing review that all the investigations which have contributed to the development of these various theories either have had a morphological basis or were of a chemical nature. Along these lines there remains a great deal to be accomplished; we especially need more exact information on the nature of the injury which is done to the chromatin and to the cell organs.

But in addition to the morphological study the general problem must be studied by other methods. The question is raised as to what is the nature of the stimulus by

[17] Joly, J., "A Theory of the Action of Rays on Growing Cells," *Proc. Roy. Soc.*, Series B, Vol. 88, 1914.

which radioactivity affects organisms. According to Verworn a stimulus is "any change in the external agencies that act upon an organism." Are the rays of radium or X-rays comparable to the electric current, for example, in the manner in which they affect protoplasm? An experiment was performed by the writer to gain information on this point. A frog's leg was set up as a muscle-nerve preparation; when stimulated electrically it was found to react normally. The nerve, and later the muscle directly, were exposed to X-rays. When the brush discharge was carefully screened away from the preparation, the X-rays were unable to cause any contraction, even of the slightest extent, as shown on the drum of kymograph. This result was obtained repeatedly. While it gives no information as to real nature of the stimulus, it indicates that the stimulus of radioactivity is not comparable in its effects with that of the electric current.

Gager has adapted Verworn's biogen hypothesis to explain the manner in which radium rays act as a stimulus to organisms, and to provide the mechanism by which the stimulation may be supposed to operate. A stimulus is any change in the external agencies that act upon an organism. Metabolism according to Verworn "depends upon the continual destruction and continual reconstruction of a very labile chemical compound," biogen, which "develops at an intermediate point in metabolism, and by its construction and destruction comprehends the sum total of metabolism." It is not a protein nor living, for a molecule can not be alive. The ratio of construction and destruction of biogen molecules under normal conditions of equilibrium is $\frac{\text{construction}}{\text{destruction}} = 1$. Therefore, "the irritability of living substance depends upon the lability of the biogen molecules.

Now, Gager remarks:

> Both the dissimilatory and the assimilatory phases of metabolism may be stimulated. The degree of dissimilatory stimulation is, for equally intense stimuli, dependent upon the following factors:
>
> (*a*) The degree of lability of the biogen molecule.
> (*b*) The rapidity of the process of restitution after the functional destruction of the biogen.
> (*c*) The absolute number of biogen molecules present.
> (*d*) The conditions of the propagation of the stimulation.
>
> A dissimilatory stimulation, or depression, may be brought about by influencing any one of these individual factors. On the other hand, the degree of assimilatory irritability is dependent upon:
>
> (*a*) The quality of the raw material available for nutrition.
> (*b*) The means for working up the raw material into a suitable form of elaborated matter.
> (*c*) The quantity of suitable elaborated matter.
> (*d*) The rapidity of the transformation of the elaborated matter from the reserve depots into the biogen molecules.
>
> An assimilatory irritability or depression may arise through influencing each of these individual conditions.
>
> Radium rays, by acting on any one of the eight factors enumerated above, may, therefore, excite or depress processes of either assimilation or dissimilation.

Further, Gager points out the probability that radium rays may not affect their stimulation "by acting directly upon the biogen molecules, or whatever the reality may be that corresponds to this term, but by acting upon other substances in the individual cells, or by modifying some process either preceding or following the elaboration of the biogen molecule." The rays may produce their effect indirectly by acting upon some non-vital constituent other than the biogen, or upon some purely chemical process. Thus does he conceive the mechanism by which the rays produce the changes which they effect on organic bodies.

It will be seen that this elaborate conception of the method by which the results

are produced does not in any way conflict with the hypotheses already stated but is really an accessory to them. Although Gager probably had no such thought in mind, his conception is in complete accord with a theory of enzyme modifiability, which at the same time presents a picture of the manner in which the radiations affect protoplasm.

Early in the investigations the question arose as to whether the effects observed in the division of the egg might not be due to a change caused by the radiation in the permeability of the cell membranes to certain substances contained in the solution in which the eggs developed. It is known that surface changes due to the alteration of permeability account for many of the phenomena connected with the initiation of development and cell division, and by analogy it was argued that to similar changes might be due the retardation of division rate as well as other departures from normal as they occur in the radiated eggs. To test this question the writer undertook a series of experiments[18] in which several different tests for permeability change were used and all gave the same result: that the X-ray effects are not to be attributed to permeability changes caused by the radiation. In the first method, the larvæ of *Arenicola,* a marine worm, were employed, for it had been found that, when these larvæ are brought into any solution that causes permeability changes, a yellow pigment is exuded from the integument; no exudation could be observed under the influence of the radiation. The second method consisted of experiments conducted with the view to producing artificial parthenogenesis, for upon the basis of the current working hypothesis, artificial parthenogenesis is due to the cytolysis of the cortical layer of the protoplasm, which in its turn is correlated with permeability changes on the egg membrane; positive results from experiments to cause this phenomenon would therefore imply permeability changes. The experiments were unsuccessful in the attempt to cause parthenogenesis. Various modifications of the indicator method were used, all with the result that substances in solution were found to enter the cell, which had been stained with some neutral indicator, in this case neutral red, after exactly the same interval in both the radiated and the unradiated control cells. This shows that the radiation is ineffective in causing permeability changes in the cell membranes. These experiments warrant the conclusion that permeability changes are not the causal factors in the events which follow radiation.

From Gager's conclusions that radioactivity is a stimulus to metabolic processes, it may be inferred that the functions, as cell division, which even remotely depend on these processes would also be affected by radiation. Such an inference is borne out by the observations[19] made by the writer on the rate of division in *Planorbis* eggs that had been exposed to X-rays, for in these experiments it was found that a light radiation served to accelerate the first one or two mitotic cycles that followed it; after that injurious effects gradually asserted themselves. A strong radiation was directly inhibitive. The cytological study of the eggs used in these experiments has not been completed, so that it is as yet impossible to correlate the observations on the living eggs with changes in the finer details of their structure. It is of course possible that we have manifested in these physiological

[18] Richards, A., "Experiments on X-radiation as the Cause of Permeability Changes," *Amer. Jour. Physiol.,* Vol. 36, 1915.

[19] Richards, A., "The Effect of X-rays on the Rate of Cell Division in the Early Cleavage of *Planorbis,*" *Biol. Bull.,* Vol. 27, 1914.

processes influences that make no impress on the morphological structure of the egg.

The case of the influence of X-rays on fertilizin, already referred to, provides another instance where the effects are without direct morphological representation. Doubtless others occur. These cases must of course be accounted for by any explanation of the effect of radiation on living organisms.

The facts, as they are at present known in regard to the effects of radioactivity on living matter, show that life processes are subject to marked changes under the influence of the radiation, a slight exposure being accelerative in most cases, while a more intense treatment is inhibitive or destructive. As a causal factor in these effects, the demonstrable injury to the chromatin of the cells is undoubtedly important; but there are also good evidences that the modifiability of enzymes under the action of the rays likewise plays a considerable part either directly or indirectly in the resulting injury.

A. Richards

Woods Hole

ARE RECESSIVE CHARACTERS DUE TO LOSS?

Since the presence-absence theory came into vogue it has become quite customary to regard recessive characters as due to the absence of something in the germ plasm on which the corresponding dominant character depends. The nomenclature of the presence-absence theory has been adopted by most writers on Mendelian inheritance, and it has afforded a useful and convenient method of expressing gametic formulas, although, as Morgan has shown, there are cases in which it leads to inconsistent results. While it is often recognized that this nomenclature is a purely symbolic scheme of indicating how certain characters behave in inheritance, the habitual employment of the system in the search for formulas which will designate by a series of large and small letters the gametic constitution of the organisms one is dealing with, has a strong tendency to influence one's views in regard to several important problems of heredity and evolution. I can not but think that the opinions of many students of genetics have been unduly influenced by their formulas. Formulas are excellent servants but bad masters. Almost involuntarily a certain interpretation is attached to their symbolism which is apt to have the practical effect of actual belief if it does not succeed in producing it.

Since the establishment of Mendel's law and its successful employment in elucidating many previously enigmatical phenomena of inheritance, heritable variations have commonly come to be considered as due to the addition or subtraction of discrete units of germ plasm, the bearers of unit characters. Professor Bateson in his "Problems of Genetics" says in regard to substantive variations that

> we are beginning to know in what such variations consist. These changes must occur either by the addition or loss of factors.

And further on he makes the following significant statement:

> Recognition of the distinction between dominant and recessive characters has, it must be conceded, created a very serious obstacle in the way of any rational and concrete theory of evolution. While variations of all kinds could be regarded as manifestations of some mysterious instability of organisms this difficulty did not occur to the minds of evolutionists. To most of those who have taken part in genetic analysis it has become a permanent and continual obsession. With regard to the origin of recessive variations, there is, as we have seen, no special difficulty. They are negative and are due to absences, but as soon as it is understood that dominants are caused by an addition we are completely at a loss to account for their origin, for we can not surmise any source from which they have been derived.

In his more recent address before the British Association, Bateson not only interprets all recessive characters as due to loss, but suggests that dominant characters may have arisen by the removal of inhibiting factors, thereby causing a "release" of the characters which previously lay latent in the germ plasm,

and producing the appearance (but only the appearance) of new variations. He says:

> In spite of seeming perversity we have to admit that there is no evolutionary change which in the present state of our knowledge we can positively declare to be not due to loss.

If we explain not only the actual disappearance of characters as caused by germinal loss, but the appearance of new characters as due to the loss of inhibitors which prevented these characters from manifesting themselves, it is theoretically possible to consider the whole process of progressive evolution as accomplished by the sloughing off of inhibiting factors. Such a doctrine which naturally reminds one of the extravagancies of the theory of emboitement might have proved quite acceptable to Leibnitz, Haller, or Bonnet, but, unless I misunderstand him, Professor Bateson has presented this view more as an illustration of the bankruptcy of present evolutionary theory than as a matter of serious conviction of his own. I will not discuss this interesting speculation further than to observe that any interpretation of variation which logically leads to such a standpoint naturally incurs a very justifiable suspicion of unsoundness. It may be that in the case of any particular variation we are unable to positively declare that it is not due to loss, but on the other hand we are unable to positively declare that most variations *are* due to loss. I think I am not going too far in stating that a germinal variation due to loss has not been proved to occur in any single case. If it is legitimate to explain the appearance of new characters as due to the removal of inhibitors, we may also explain the apparent loss of a character as due to the advent of inhibitors. It is surely justifiable to assume that inhibitors can come into an organism somehow if we are permitted to make such frequent use of their disappearance in accounting for the origin of new variations. The plain fact is that we know practically nothing of the changes in the germ plasm which we postulate as the causes of variability. It is easy to assume the existence of an inhibitor to bring any particular variation into line with one's general theory, but such explanations are purely formal and therefore of little scientific value.

While few would be inclined to follow Bateson in his rather paradoxical interpretation of dominance, the doctrine that recessiveness is due to loss is coming to be quite prevalent among workers in genetics. One of the chief reasons for regarding so much of the variation that has arisen among domestic animals as caused by the loss of factors is the fact that the crossing of different varieties often produces a reversion toward the ancestral type. If we regard the ancestor of our races of domestic mice for instance as possessing a full complement of factors, and assume that the different varieties have arisen by the dropping out of one or more factors in this variety, and one or more other factors in that variety, then when these varieties are crossed the hybrid may possess all the factors of the original ancestor and hence show a reversion to type. On the basis of this assumption one can make out gametic formulas for the different varieties of a species, test them by breeding experiments, and thus verify their correctness. Gametic fomulas obtained in this way doubtless symbolize a truth in regard to the germinal constitution of the organisms in question. The value of such formulas is no longer a matter of doubt, and is quite independent of the various interpretations that can be made concerning the nature of the symbolism, just as chemical formulæ are of value quite irrespective of the various theories of the constitution of atoms.

Consider the origin of a black mouse according to the presence-absence hypothesis. We may explain the origin of a black mouse by saying that it is caused by the absence of the agouti or ticking factor that breaks up the color of the hair into bars. Gray is therefore black plus an agouti factor. But does it follow that because we can interpret the facts in this way, and interpret them consistently so far as breeding experiments are concerned, the change that has taken place in the germ plasm that produced a black mouse was really a loss? Such a change is frequently

assumed to be the result of an actual loss of a little discrete unit of some sort in the germ plasm. De Vries has interpreted recessiveness as due to the latency or loss of potency of pangens, but we may also assume that the germinal basis of the character in question has undergone a change of such a character that without becoming inactive it ceases to function in its usual way. The agouti factor (commonly designated by G) may be regarded as dependent on a part of the germ plasm, a section of a chromosome possibly, which when present causes the barring of color in the hair. When a black mouse arises we may suppose that something takes place in G. It is not necessarily a change in the direction of either chemical or organic simplicity any more than it is necessarily a loss of substance. The fact that the modified condition is recessive to G proves nothing whatsoever in regard to the nature of the transformation that has occurred in the germ plasm. Assuming that G is not actually lost, but modified into another kind of substance g, the recessiveness of g may be due to the fact that its activity is manifested in a different way, relative slowness of its metabolism, or to various other conceivable causes.

There is, I believe, no good reason for considering the recessiveness of a character as due to the relative simplicity of its germinal basis. Many variations of a minus character are recessive, but there are numerous exceptions to this rule, as is illustrated by the dominance of the hornless condition in cattle, the short tail of Manx cats, and the lack of beards in certain kinds of wheat. Suppose we have two allelomorphic units (assuming for the present that there are such things as germinal units) A and A′, one of which tends to produce a relatively simple development of a part and the other a relatively complex development of a particular part of the body. The one A calls forth, say, simple horns, the other branched horns. A and A′ presumably differ chemically, and the development of the part in question depends not upon A or A′ alone, but upon how these agencies affect other parts of the body during development. Will the simpler substance or organic unit call forth the simpler structure in the adult body? Inasmuch as the development of any organ involves activities in which a larger number of elements are concerned it seems not at all improbable that the simpler substance or unit might conspire to produce the more complex organ. Now suppose that the forked horn proved to be dominant over the simple horn. What conclusions would we be entitled to draw from this fact concerning the germinal basis of these characters? Obviously none.

Whether we interpret a variation as a gain or a loss is in most cases a purely arbitrary matter. In sugar corn there is a loss of starch but there is a gain of sugar. Does sugar corn therefore represent a plus or a minus variation? Consider the familiar cases of rose comb and pea comb in poultry. Both of these variations are dominant over the primitive condition of single comb. Yet both breeds carry the basis for the production of single comb in their germ plasm. It is commonly assumed that both conditions represent single comb plus something. We may suppose that in a certain chromosome a change has taken place which results in the development of rose comb. This change, for all that we know, may be due to the loss or impairment of a portion of germ plasm, or it may be due to a change not properly describable as either a gain or loss. We may regard rose and pea comb as more or less pathological deviations based on germinal defect, as true progressive variations, or simply as normal variations neither progressive nor retrogressive. So far as complexity of structure is concerned it may be a matter of dispute whether rose comb, pea comb, or single comb represents the higher grade of development.

But, it may be asked, are not color varieties commonly due to loss, and is not this obviously the case with albinism? In many varieties there has certainly been a loss of pigment, but has there been a dropping out of factors? It by no means follows. The factors represented by small letters in our color formulas are by no means missing entities. They are changed so that they occasion a diminished production

of certain pigments, but in other respects they may be as potent as before. The albino does not produce pigment, but there may be other substances in the place of pigment that would distinguish the albino as a positive variation when judged by other standards. The animals whose gametic formulas contain a number of small letters are not necessarily more imperfect or perhaps I should say incomplete than their congeners which carry a large number of dominant characters.

Of course there may be varieties due to losses of germinal material. Considering the complex mechanism of mitosis, and the opportunities afforded for the loss of chromatin during this process, such variations are not improbable *a priori*. But there is not the slightest warrant in the fact of recessiveness *per se* for the doctrine that all recessive variations are produced by this method. The origin of so-called unit characters may depend, for the most part, not upon germinal loss or gain, but simply on transformation. Viewed in this simple and natural way the appearance of a new dominant character is not an event to be marvelled at. Dominant and recessive characters not improbably owe their origin to much the same causes. At least we do not know that they do not. Concerning the real causes of variations of any kind we know very little more than we did when Darwin commented on our profound ignorance of this subject. It is therefore premature to pin our faith to any particular theory of the origin of variation and especially to draw far-reaching conclusions regarding evolution on the basis of such an interpretation. We may conceive variability as due to germinal losses or gains for the sake of our formulas, and there may be little harm in so doing so long as it is clearly realized that the procedure is a purely arbitrary and schematic method of recording certain facts of inheritance. But when we make the serious attempt to apply the conception to what actually takes place in the germ plasm we encounter a fruitful source of fallacies.

S. J. HOLMES

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ERNST GRIMSEHL[1]

ON October 30, 1914, Ernst Grimsehl fell near Langemarck in the bitter fighting along the Yser line. Only two days before he had received the iron cross. Although he was in his fifty-fourth year, yet he responded voluntarily and full of enthusiasm to the call to the colors as an "Oberleutnant der Landwehr." On October 1 he marched with the 213th regiment across the Belgian frontier. For only a few weeks was he permitted to fight for his country which he so dearly loved. He died, as so many others at his side, without living to see the victory which he so confidently hoped for.

In his death the German educational system loses a personality which was unique in its character and therefore can not be replaced. All his thoughts and efforts were directed to this ideal of placing physics teaching on a firmer basis and of bringing it nearer and nearer to perfection. His friend, A. Keferstein, has in the *Unterrichtsflättern* sketched the character of his work with beauty and conviction. He says in part:

> His preeminent manual dexterity and his thorough knowledge of the instrument-maker's art, which as a student he had gained in his vacation days from the masters of the art, qualified him for the creation of clean-cut models of apparatus. These he tried out at every point till he had corrected by his masterly hand their first faults and made them respond to his every wish.

Of his original inventive skill as an experimenter, numerous publications bear witness; one must have watched him getting ready for an experimental lecture such as he was wont to give almost every year at the spring meeting of the Association for the Promotion of Instruction in Mathematics and the Natural Sciences, in order to gain the secret of this skill. He was tireless and enthusiastic in his efforts to perfect his arrangements, often by hours of labor in a strange place and in a

[1] Translated from *Zeitschrift für den Physikalischen und Chemischen Unterricht*, January, 1915, and read at the seventieth meeting of the Eastern Association of Physics Teachers by N. Henry Black.

strange laboratory, so that he might manage things there just as in his own rooms. Everything must be carefully tested before he began his lecture. So it came about that his demonstrations became the star performances and attractions of each of these meetings.

It is difficult to enumerate all the pieces of apparatus for purposes of instruction and investigation which we owe to him. These are almost all published in our magazine, in which he took a lasting interest. Even the first volume in 1888 contained a report of his paper in which he published a new method of measuring the intensity of a tone. In the second volume there appeared the first original investigation from his hand, in which he described two pieces of apparatus for detecting the nodal points and internodes in a sounding column of air. His last lecture, which he gave in the spring of 1914 upon a new and simple means of showing the interference of light, he had also intended for our magazine. However, before he came to write it down, the war had pressed into his hand the sword instead of the pen.

Of his books only two may be mentioned here: the large "Lehrbuch der Physik"[2] which has in five years gone through three editions and the "Didaktik und Methodik der Physik"[3] (a part of Baumeister's Handbook) which in spite of its brevity and its strong personal color, is rich in valuable advice and fruitful ideas.

Death has brought his work to an untimely end, but the influence of this creative work will live after him and will assure for him a grateful memory among his followers as well as in the history of the teaching of physics.

SCIENTIFIC NOTES AND NEWS

Dr. Paul Ehrlich, the distinguished German pathologist, director of the Royal Institute for Experimental Therapeutics in Frankfurt a. Main, died on August 20, at the age of sixty-one years.

[2] B. G. Teubner, Leipzig.

[3] C. H. Beck'sche "Verlagsbuchhandlung," München.

Dr. Carlos J. Finlay, a leading physician of Cuba, known for his advocacy of the theory that yellow fever is transmitted by mosquitoes, died on August 20, at the age of eighty-two years.

It is announced that in consequence of the war, the meeting of the Australasian Association for the Advancement of Science, which had been arranged to take place in Hobart in January next, has been postponed for a year.

Dr. David Bancroft Johnson, president of Winthrop Normal and Industrial College, of Rockhill, S. C., has been elected president of the National Education Association, in succession to Dr. David Starr Jordan, chancellor of Stanford University.

During the San Francisco meetings of the American Association for the Advancement of Science, there was formed a Pacific Coast Branch of the American Society of Zoologists. The officers elected at this meeting were:

President: V. L. Kellogg, Stanford University.
Vice-president: R. M. Yerkes, Santa Barbara.
Secretary and Treasurer: Joseph Grinnell, University of California.
Executive Committee: C. O. Esterly, Occidental College; Barton W. Evermann, California Academy of Sciences; Charles L. Edwards, Los Angeles; J. Frank Daniel, University of California; Harold Heath, Stanford University.

At the same meeting there was formed a Pacific Coast Branch of the American Society of Naturalists with the following organization:

President: Barton W. Evermann, California Academy of Sciences.
Vice-president: John F. Bovard, University of Oregon.
Secretary: Ellis Leroy Michael, Scripps Institute for Research.
Treasurer: L. L. Burlingame, Stanford University.
Executive Committee: Trevor Kincaid, University of Washington; Harry Beal Torrey, Reed College; Frank M. McFarland, Stanford University.

The society will take the place of the local biological societies of the Pacific Coast.

The Biological Society of the Pacific met at the Hotel Sutter, San Francisco, on August 4, for its annual meeting. The ad-

dress of the evening was given by Dr. Harry Beal Torrey, of Reed College, on "Research and the Elementary Student of Science." At this meeting the Biological Society voted to drop its organization in favor of the newly organized Pacific Coast Branch of the American Society of Naturalists.

THE forty-third annual meeting of the American Public Health Association, the fifteenth annual conference of the Sanitary Officers of the State of New York, and the annual meeting of the New York State Sanitary Officers' Association, will be held in Rochester, N. Y., September 6 to 10.

BEFORE the American Public Health Association on Tuesday evening, September 7, the presidential address will be delivered by Professor William T. Sedgwick, of the Massachusetts Institute of Technology, his subject being "Achievements and Failures in Public Health Work." Other speakers at the meetings are Dr. Hermann M. Biggs, New York state commissioner of health; Dr. W. C. Gorgas, surgeon-general United States Army, Washington, D. C., and the Hon. William C. Redfield, secretary of commerce.

DR. ALBERT EULENBERG, the distinguished neurologist of the University of Berlin, has celebrated his seventy-fifth birthday.

DR. VON STRÜMPELL, professor of medicine at Leipzig, has been elected rector for the ensuing year.

SIR A. SELBY-BIGGE, permanent secretary of the British board of education, has been appointed special secretary to the committee of the privy council for the organization and development of scientific and industrial research in Great Britain.

THE gold medal of the Company of Dyers, London, has been awarded to Professor A. G. Green, University of Leeds, and to Mr. W. Johnson, a research student of the University of Leeds, for research work in connection with the art of dyeing.

THE trustees of the American Medicine Gold Medal award have selected Surgeon-General Rupert Blue, of the Public Health Service, as the American physician who has done most for humanity in the domain of medicine during 1914, and the medal has been awarded to him for his work in national health and sanitation.

THE governor of Indiana has appointed a commission to investigate the causes and prevention of mental deficiency in the state. The medical members of the commission are Drs. George F. Edenharter, superintendent of the Central Indiana State Hospital, Indianapolis; Samuel E. Smith, superintendent of the Eastern Indiana State Hospital, Richmond; Charles P. Emerson, dean of the Indiana University School of Medicine, Indianapolis; Walter C. Van Nuys, superintendent of the Indiana Village for Epileptics, Newcastle; and Dr. George S. Bliss, superintendent of the State School for Feeble-minded Youths, Fort Wayne.

THE British secretary of state for the colonies has appointed a committee, presided over by Mr. A. D. Steel Maitland, parliamentary under-secretary of state for the colonies, to consider and report upon the present condition and the prospects of the West African trade in palm kernels and other edible and oil-producing nuts and seeds and to make recommendations for the promotion, in the United Kingdom, of the industries dependent on them. Mr. J. E. W. Flood, of the colonial office, is secretary of the committee.

DR. J. A. UDDEN, geologist of the bureau of economic geology in the University of Texas, has been appointed acting director of the bureau, the former director, Dr. Wm. B. Phillips, having resigned to become president of the Colorado School of Mines.

SIR AUREL STEIN, who has been making explorations in Central Asia, has arrived safely as Kashgar.

MR. CHARLES P. LOUNSBURY, chief of the department of entomology of the South African Union, expected to arrive in San Francisco about September 1. He has been spending some time in Australia and other points en route in furthering the interests of his department. He expects to be in America for several months.

The Reverend Alphone Schwitalia, S.J., professor of biology at St. Louis University, and two other members of the party, have returned from a medical inspection trip to British Honduras. Dr. Edward Nelson Tobey, assistant city bacteriologist and a lecturer at the university, also was a member of the expedition, but it is feared he perished with the steamer *Marowijne,* which has not been heard from since the West Indian hurricane swept through the Yucatan channel on August 13.

A memorial to Johann C. Reil, the anatomist, has been erected in Halle. It stands in front of the university clinic, the seat of his labors until called to Berlin in 1810. He died in 1813, aged fifty-five years.

The death is announced of Dr. B. Fisher, professor of hygiene and bacteriology in the University of Kiel.

The Paris Academy of Medicine has received a legacy from Dr. M. Sigaut of 8,000 francs to be awarded for a research on cancer of the digestive tract.

The exhibit arranged by the New York State Museum for the department of mines and metallurgy at the Panama-Pacific International Exposition was awarded a grand prize, besides one medal of honor, five gold medals, fifteen silver medals and nine bronze medals.

The Coast and Geodetic Survey informs the American Geographical Society of some recent significant soundings by the steamer *Pathfinder* in the southwest part of the Philippines area. The Cagayanes, Cavilli and Arena Islands, Tubbataha and Maeander Reefs, in the Sulu Sea, are apparently coral capped summits of a submerged mountain range extending for 200 miles southwesterly from the southwest part of Panay Island. They rise from depths of 6,000 to 12,000 feet with a stupendous submarine slope. The soundings indicate that this range divides the Sulu Sea into two deep basins by joining the shelf or plateau extending northwest of Borneo and east of Balabac Strait. Bancoran Island and Moyune Reef are elevations at the south end of the northwest basin. The Tubbataha Keys and Maeander Reef are the only elevations without vegetation. They are steep faced, similar in structure and consist of an accumulation of dead corals, coral rock and coral sand cemented into a greater or less degree of compactness. The pounding of the sea has accumulated the coral sand in the center to an elevation of five or six feet.

The Field Museum of Natural History has recently acquired a large collection of vertebrate fossils from the asphaltum beds of southern California. This collection consists of more than two thousand specimens varying from skeletons to single bones. Among them are mounted and mountable skeletons of the saber-tooth tiger (*Smilodon*) and the large wolf, *Canis dirus,* together with numerous series of skulls and skeletal parts of these animals. There are also skulls of *Megalonyx, Bison, Teratornis, Gymnogyps* and *Cathartes.* Other genera represented are *Felis, Camelops, Mastodon, Equus, Cervus* and *Antelocapra.* Most, if not all, of these specimens are of Pleistocene age. For this valuable collection the museum is indebted to the generosity of Messrs. E. E. Ayer, M. A. Ryerson, W. R. Linn and E. B. Butler, members of its board of trustees.

There has just been issued by the Bureau of Standards a paper describing briefly the methods of calibrating and using bomb calorimeters, such as are used in determining the amount of heat available from a given weight of coal or coke or other combustible. The amount of heat which can be obtained depends largely upon the kind and quality of fuel. When purchased in large quantities, therefore, a fuel is commonly tested to determine the amount of heat available per pound, and the price paid depends upon the results of these tests. The instrument used for such tests is called the bomb calorimeter and consists essentially of a steel shell or "bomb" in which a small weighed sample of the fuel can be burned in pure oxygen gas. The bomb is immersed in a known amount of water before the sample is ignited, the heat produced warms the water, and by suitable measurements of the change of temperature the amount of heat can be calculated. Provision is made

by the Bureau of Standards for standardizing bomb calorimeters by means of standard samples of certain pure materials, viz., sugar, napththalene and benzoic acid. By burning known amounts of these substances in the bomb the observer determines the amount of heat required to raise the temperature of the bomb together with the proper amount of water one degree. This being determined the amount of heat furnished by a given sample of coal burned in the same bomb with the same amount of water can be found. Thus these standard samples, which are sent all over the United States, serve as standards of heat and make it possible to get the same results from tests made anywhere in the country, much as the use of the standards of length and of mass makes a yard or a pound the same in all parts of the country. Copies of this paper known as Circular No. 11, "Standardization of Bomb Calorimeters," may be obtained without charge upon application to the Bureau of Standards, Washington, D. C.

THE *Journal* of the American Medical Association reports that Mr. James Berry, who is at the head of a British hospital mission at Vrnjachka Banya, has collected from official sources figures which show that ninety-three Serbian physicians have died out of a total of 387 alive at the beginning of the war. Of these, no fewer than eighty-two succumbed to typhus fever, and only one was killed in battle. These figures contrast remarkably with those of the recent Turkish war in which Serbia lost only two physicians. Of the foreign physicians who have come to her aid in this war, thirty-five have died from typhus or typhoid fever. They include three British, four American, two Belgian, several Greeks, and six others.

UNIVERSITY AND EDUCATIONAL NEWS

DR. JOHN LEE COULTER has been appointed dean of the College of Agriculture and director of the Experiment Station of the West Virginia University. He goes from the George Peabody College, and will take the place of E. D. Sanderson, who resigned about a year ago.

AT the Johns Hopkins University, the degree of bachelor of science in education has been established in connection with the college courses for teachers and the summer courses. The degree will be open to men and women. The regulations concerning the work for the new degree will be determined by an advisory committee of the faculty. The title of director of the college course for teachers and of the summer courses has been assigned to Professor Edward F. Buchner.

DR. ORIN TUGMAN, of the staff of the research laboratory of the Eastman Kodak Company, has been elected associate professor of physics at the University of Utah.

DR. L. CHAS. RAIFORD, of the department of chemistry of the University of Chicago, has been elected professor of chemistry in the Oklahoma Agricultural and Mechanical College.

DR. J. A. MENZIES has been appointed professor of physiology in the University of Durham College of Medicine, Newcastle-upon-Tyne.

DISCUSSION AND CORRESPONDENCE

ANOTHER REASON FOR SAVING THE GENUS

I AM writing to second Dr. F. B. Sumner's plea for the saving of the genus.[1] I am sure he has the sympathy of the great mass of workers in non-taxonomic biology. Leaving aside the question of expressing relationship in the generic name which Sumner has so well stated, there is another point that he has not sufficiently emphasized. It is by the genera that animals and plants are catalogued. In the *Nautilus,* Vol. 28, February, 1915, the writer made this plea. I illustrated it by the form on which I had been working for the past eight years, the genus *Lymnæa.* I quote the following passage from that paper:

> The most recent classification of this group is that of F. C. Baker in his admirable "Lymnæidæ of North and Middle America" (Chicago Academy of Sciences Pub. No. 3, 1911), p. 120. Whereas the older classifications considered shell characters alone, this author "proposed to classify the

[1] "Some Reasons for Saving the Genus," SCIENCE, Vol. XLI., No. 1068, p. 899.

Lymnæids by the characters of the shell, genitalia (shape of prostate, relative size and form of the penis and penis-sac) and radula.''

On a basis of these criteria he has split the genus *Lymnæa,* as defined by Haldeman, 1840, Gould, Binney, 1868; Dall, 1871; Tryon, 1872 and 1884, and more recently by Dall in 1905, into six genera: *Lymnæa, Pseudosuccinea, Radix, Bulimnæa, Acella* and *Galba.* He has done this mainly by raising a number of subgenera and sections of former authors to generic rank. I wish to ask this question: Is this at the present time justifiable? (1) Baker lists 103 species and varieties of the old *Lymnæa* in this work. Of but 33 have anything of the anatomy, radula and genital organs been studied. Therefore the shell characteristics are the important ones after all. (2) All these new genera are based largely on quantitative characters. The only qualitative character mentioned is the radula and this is given quite a subordinate place in the classification. (3) In his diagnosis of the genus *Galba* in his key he states that the ''Penis'' (epiphallus) is shorter than the ''penis-sac'' (penis). However, for two of the species of this genus the epiphallus is longer than the penis. (See Baker, pp. 263 and 277.)

In the mind of the writer our present knowledge will not allow us to make a comprehensive classification of the Lymnæids based on the anatomy of the snail. We know too few species well. On the other hand, the shell characters alone in a mollusk with such a generalized form of shell as have the Lymnæids are not characters on which one can base much reliance. On account of these reasons the writer would make the recommendation that the old genus *Lymnæa* should be retained in the sense that it has been used for the past seventy years.

In the *Nautilus* for June, 1915, Mr. F. C. Baker answered the writer in an article entitled "On the Classification of Lymnæids." I think this may be taken as the typical attitude of a taxonomist. He said:

> The writer can by no means agree with the statement made twice in this paper (*loc. cit.*) that generic names should not be added unless based on undebatable grounds, because of the inconvenience of the cataloguer. If this criticism should be recognized we should revert to the use of many of the older names in the Pulmonata as well as in the Naides.
>
> It is recognized, of course, that generic subdivisions can be overdone, but in the advancement of science the convenience of the cataloguer or teacher is not considered.
>
> We welcome all additions to knowledge and we know full well that the work of yesterday is rendered obsolete by the work of to-morrow, but the writer can not see how the reduction to subgenera and sections of the names used as genera and subgenera in the monograph in question advances our knowledge of the family any more than the raising of a number of subgenera and sections to generic rank, as Colton believes the writer to have done in his monograph. This rather resembles a game of see-saw.

This whole discussion hangs on the question, is it necessary to change generic names to advance our knowledge? The writer believes that to change generic names without an overwhelming amount of evidence in favor of the change is hindering instead of advancing science. Species and minor groups, on the other hand, can not be too much subdivided. It is an advance to describe every variation that can be distinguished. Of this work Bateson[2] says:

> They will serve science best by giving names freely and by describing everything to which their successors may possibly want to refer, and generally by subdividing their material into as many species as they can induce any responsible society or journal to publish.

In conclusion, generic names are those by which animals are catalogued, therefore should not be changed without overwhelming evidence in favor of the change. This value of the generic name has not been sufficiently emphasized.

Harold S. Colton
Zoological Laboratory,
University of Pennsylvania

The End of Cory's Shearwater

Cory's shearwater (*Puffinus borealis*) does not exist. It seems a pity to abolish so time-honored and respected a species; but the truth is that it already stands abolished, and nothing is required but the awakening of us American bird-men to the fact. It is indeed a token of provincialism on our parts that this remarkable error should have gone for thirty-four

[2] ''Problems of Genetics,'' p. 249.

years uncorrected. For Cory's shearwater, described in 1881, was not a new bird, but an old bird with a new name. Its range is not unknown, and its nesting-habits and eggs have long been familiar to naturalists. As I have for some time suspected, as Howard Saunders stated positively away back in 1889, and as Godman in his "Monograph of the Petrels" (pp. 94–98, Part II., 1908) has established, *Puffinus borealis* is a synonym of *Puffinus kuhli,* the Mediterranean great shearwater, a common Old World bird which has been well known for generations.

My first intimation of this fact I found in Howard Saunders's "Manual of British Birds," wherein, on p. 716 (first edition), in treating of the great shearwater, he remarks:

> In the Azores, as well as on the islets near Madeira and the Canaries, the resident species is *P. kuhli* (identical with *P. borealis* of Cory), which visits the western coasts of France and the Peninsula, and is abundant throughout the Mediterranean; the latter species is of a much paler brown on the upper parts, and has a yellow-colored and deeper bill.

This was startling, since I knew that Saunders was not a man for unguarded statements; but at the same time it seemed incredible that an assertion of this kind in a standard bird-book should have remained unnoticed and uninvestigated by American bird-men for twenty years; and as Cory's shearwater still held its place in all our bird-books, I was puzzled. I recalled with intense regret the accidental loss of a specimen of *P. kuhli* which my father and I had once collected off Sardinia; and I set about trying to get together some skins of these big pale-billed shearwaters from both sides of the Atlantic, for comparison. Rosenberg, in London, wrote me that he had one skin only of *P. kuhli.* I meant to order this, and also to write to a bird-stuffer we knew in Cagliari, Sardinia; but other matters intervened, and I let the whole thing slip.

Then, hearing of Godman's "Monograph of the Petrels," I supposed it a matter of course that I should there find the question definitely settled. For some time I had no chance to see this work; and meanwhile I noticed that the latest-revised bird books in America were still hanging on to Cory's shearwater. Godman, then, had confirmed its standing as a distinct species? Apparently, this must be so. Yet the fact that in all these years a large shearwater breeding abundantly in the Azores had not been recorded even as a wanderer from our Atlantic coast seemed in itself an exceedingly suspicious circumstance. Strong-winged sea-rovers like these should find no barrier between the "Western Islands" and the New England fishing-banks.

My doubts continued until, in June of this current year, 1915, I was enabled through the kindness of the secretary of the Boston Society of Natural History to examine Godman's monograph. There I find the matter satisfactorily settled, in conformity with Saunders's statement and my own misgivings. Under the head of *Puffinus kuhli,* Godman (Part II., p. 96) says:

> Specimens from the eastern coast of North America have been described as *Puffinus borealis* by Mr. C. B. Cory, but I can not find any difference between individuals from the coast of Massachusetts and others from the Atlantic islands.

In his synonymy of *P. kuhli* he includes Cory's *P. borealis.*

It would seem unnecessary, not to say presumptuous, for us to question this determination, or wait to make further comparison of specimens before admitting that our "Cory's" shearwaters are simply Mediterreanean great shearwaters on their annual post-breeding-season pilgrimage to the fishing-grounds on the western side of the Atlantic. It must be noted, however, that the Azorean and Canary Islands birds have been found to be subspecifically distinct from those breeding in the Mediterranean, differing mainly in the smaller amount of white in the lining of the outer primaries. The Atlantic islands bird has been described by Hartert as *Puffinus kuhli flavirostris,*[1] and

[1] The name of *Puffinus flavirostris* was first used by Gould in 1834, for specimens of the Mediterranean (Azorean) shearwater from the Cape Seas. It appears that the species not infrequently wanders far southwards.

Godman states that the differences are inconstant, that a complete gradation evidently exists between the extreme types, and that the two forms can not be considered as more than subspecifically distinct. It is undoubtedly the Atlantic subspecies *flavirostris* which regularly visits our coasts. According to the American system, Number 88 of the A. O. U. Check-list, 3d edition, should evidently stand as *Puffinus kuhli flavirostris* Hartert, Yellow-billed or Azorean Shearwater. Gerald H. Thayer

Monadnock, New Hampshire,
June 21, 1915

IRON BACTERIA

It has been known for many years that some of the higher bacteria are concerned in the precipitation of ferric hydroxide from iron-bearing waters. Thus *Crenothrix polyspora,* which is often abundant in city water pipes where the water contains a small percentage of iron, is held to be responsible for the frequent turbidity of the water in such places, due to the separating out of ferric hydroxide, and also for the filling of pipes with ferric hydroxide which sometimes occurs. Certain other forms, like *Chlamydothrix ochracea, Spirophyllum ferrugineum* and *Gallionella ferruginea,* have been abundantly encountered in surface iron-bearing waters, where they form thick gelatinous deposits of yellowish-brown scum.

More recently certain lower bacteria have been described which show the same characteristics with regard to the precipitation of ferric hydroxide and which seem to be very abundant in surface waters.

Different investigators have attempted to explain this phenomenon in different ways. Some, notably Winogradsky and Lieske, believe that there is an oxidation from ferrous to ferric iron and that this furnishes the bacterial cell with energy. Lieske also claims that, as the iron is usually in solution as ferrous bicarbonate, the carbon dioxide set free by the oxidation is used by the cell for building up its tissues. Other investigators, like Molisch and Ellis, state that the precipitation of ferric hydroxide is a simple chemical phenomenon and is not connected with the life processes of the cell. They believe that the accumulations of ferric hydroxide upon these organisms or upon their remains is purely mechanical. At the same time they admit the association of iron bacteria with iron-bearing waters, and realize that ocherous scums in such waters consist largely of bacterial remains.

Most of the investigations on iron bacteria have been made in Europe and relatively few investigators have concerned themselves with the problem. At the present time the writer is engaged in a field and laboratory study of these organisms and it is hoped that this work may throw some further light on the peculiar phenomena connected with their activities.

During the field work it has been found that iron bacteria are present in almost all iron-bearing waters, surface as well as underground. *Crenothrix* and *Spirophyllum* have been found in city waters, *Spirophyllum* and *Gallionella* have been found in the underground workings of mines even to a depth of several hundred feet, while *Chlamydothrix* and *Spirophyllum* have been found in surface iron springs and bogs. It seems that the bacterial flora of different localities varies. In some localities iron-bearing waters have a mixed flora, while in other localities one finds almost pure cultures of one or another of the higher iron bacteria. Thus some iron springs contain big, fluffy masses of *Chlamydothrix,* while others contain a brownish-yellow deposit consisting almost entirely of *Spirophyllum.* Some mines contain in their underground workings only *Spirophyllum,* while others contain mixed cultures. The reason for this difference is not known, but it is possible that the character of the salts in solution influences the bacterial flora.

Lower bacteria, of the coccus or bacillus forms which precipitate ferric hydroxide, are more difficult to study than the higher iron bacteria, as they can be distinguished only by their physiological activities. In order to determine the general distributions of such organisms in nature various iron solutions were inoculated with different types of water and soil and it was found that ferric hydroxide was

precipitated from these solutions after an interval of time which varied with the different inoculations. These experiments show the almost universal presence of organisms capable of precipitating ferric hydroxide. In order to show definitely that organisms were responsible for this precipitation, sterilized duplicates of the different cultures were prepared and these did not show any precipitation.

It was found likewise that solutions of different iron salts are affected in a different manner during these inoculations. In some solutions no precipitate forms, perhaps because the salts used inhibit bacterial growth. In other solutions, notably solutions of inorganic salts, the precipitation of ferric hydroxide takes place almost immediately, due to oxidation by oxygen present in the solvent. Certain solutions were kept under anaerobic conditions by passing carbon dioxide through them and it was found that in some of them ferric hydroxide was precipitated while in others no precipitation took place. In general the experiments have shown that precipitation may take place from solutions of ferric, as well as ferrous salts.

Up to the present the writer's attempts to isolate the lower bacteria present in soil and water, which are responsible for the precipitation of ferric hydroxide, have been unsuccessful, but it is planned to prepare and to experiment with various kinds of media in order to bring about this result. Until this isolation has been accomplished it will not be possible to study their morphology.

The morphology of the higher iron bacteria, unlike that of the lower, can be studied very readily, as they can easily be distinguished from other types due to their characteristic form. While it is comparatively easy to cultivate such forms as *Crenothrix* and *Chlamydothrix* in the laboratory, it is extremely difficult to isolate them from other forms in order to study their physiological processes. This is because of the fact that numerous lower bacteria find lodgment on the threads of these higher types, and are continually transferred with them.

One of the principal points of interest in connection with these investigations has been to note the relation that the iron bacteria might have to the formation of iron ore deposits. It has been claimed that they play an important part in the formation of numerous small deposits of bog iron ore, and it seems possible that their activities may in part be responsible for extensive beds of sedimentary iron ore as well. Further, the fact of finding iron bacteria in underground mines opens the possibility that certain underground deposits of iron ore have been formed by them.

The writer hopes soon to publish a detailed report on the results of these various investigations.

E. C. HARDER

U. S. GEOLOGICAL SURVEY

A TYPICAL CASE

I HAVE read your correspondent's letter on "A Typical Case Exemplified" in the number of SCIENCE dated May 21, 1915, and I have been struck by certain parallels and differences in his case and my own case. I feel that perhaps my case is worthy of citation.

I, too, completed my work for the doctorate in one of the oldest and largest of eastern institutions and, after having spent a year as instructor there, came to the northwest at the invitation of the president of the institution and the head of my department with the promise that I should have a *fair* opportunity for original investigation. During my sojourn of five years here, I have encountered no such conditions as cited by your correspondent and know of no such conditions in any western institution with which I am familiar. Before completing my work for the doctorate, I spent my time in three western institutions as student and instructor. In all of them, I found the research spirit freely encouraged. In my experience I have never been told that research was personal and that I must bear the expense and take time for it from my recreation and sleep. As a rule, the man imbued with the research spirit is not likely to allow such obstacles to stand in his way without surmounting them and is likely

to suffer from a want of recreation and lack of sleep.

The problem before the western institution is quite different from that which confronts the older eastern institutions. The western institutions are in a state of flux and developing with the country. They are dependent upon the legislatures for part of their income. The legislatures are, as a rule, generous, but frequently the funds available for appropriation are not sufficient to meet all demands and some one must suffer. The difficulty of finance is one not characteristic of western institutions but nation wide. It thus happens that funds for investigation requiring elaborate apparatus and equipment are not always available. Such conditions can not be laid at the door of the administration which, as a rule, does the very best it can under given conditions.

Considerable space was devoted to the prominence of extension work and the popular place it occupies in the institutions' activity. It is true that extension work occupies an important position, but in no sense does it overshadow the research worker. The two go rather hand in hand. The extension lecturer should be a man possessing the research spirit if his extension work is to be of any educational value. Extension work is a legitimate function of a university in that it extends the truth, for no amount of exploration for truth is worth the effort it costs but extension be the ultimate end.

Undoubtedly your correspondent's case is a bona fide one, but to assert that such conditions which he cites are characteristic of our western institutions is fallacious. There are unquestionably institutions of the character he describes but they are not localized in any particular section of the country. No man need affiliate himself with such an institution, for the report of the Federal Educational Commission and other literature should give some evidence in one way or another of such a condition.

Our western institutions can not entirely disregard the research spirit, for they are looking towards a wider recognition in the educational world; and such recognition can come only from the attainments of the individuals composing the teaching staff. To suppress the spirit of original investigation is to cast them into utter oblivion in the field of higher education.

The thing with which I particularly wish to take issue in your correspondent's letter is the statement that research is impossible in the western university. The thing which I wish to emphasize is that no such condition is characteristic of the western institution, that sporadic cases do exist I do not deny, but such cases are not confined to the west but are scattered nation wide.

B. J. Spence
University of North Dakota,
Grand Forks, N. Dak.

SCIENTIFIC BOOKS

The Determination of Sex. By L. Doncaster, Cambridge University Press, 1914. New York, G. P. Putnam's Sons.

Professor Doncaster's book gives a popular account of recent work on sex determination, avoiding as far as possible technicalities which might embarrass the untrained reader. The author has succeeded in his difficult task of presenting a considerable body of matter, much of it controversial, to a general audience. He points out that determination of sex means not the control of sex (*i. e.*, the production of sex at will) but the study of the causes that lead to the appearance of males and females. "We may discover the causes of storms or earthquakes, and when our knowledge of them is sufficiently advanced we may be able to predict them as successfully as astronomers predict eclipses, but there is little hope that we shall ever be able to control them."

Doncaster is not a little concerned that the use of the word cause in connection with sex determination be clearly understood. A factor *A* may be invariably followed by a condition *E*, but between the two there may be a chain of events *B, C, D*. Should *B* or *C* or *D* be produced in some other way this would also lead to *E*. Similarly for sex, a female results when certain conditions are realized in the egg, a male when other conditions prevail. This general philosophical point of view will,

of course, be readily conceded as an article of broadmindedness; meanwhile we must wait for a specific case where it can be shown that males and females may be turned out in these different ways. For, while no one doubts that such things as blue flowers, let us say, may be due to different pigments that go back in origin to different factors, yet so far as known to the reviewer there is no case in the whole Mendelian literature where it has been *proved* that the same (not merely similar) product is the result of different factors.

A somewhat similar question comes up in connection with certain attempts that have been made to account for departures in the sex ratio on the basis that the sex factor has become "weakened." The result would lead to complete mix-up of the chromosome relations and would lead to chaos in subsequent generations if the same kind of "weakness" kept up. In contrast to such speculations the relative constancy of the chromosome number must appear an impressive fact. Doncaster himself, while lending a sympathetic ear to those who find difficulties in applying the chromosome interpretation to sex determination, takes in general the stand with which most of us will heartily agree, namely, to hold fast to what has been most clearly demonstrated and not let the fact that there are still unsolved problems confuse the issue. Progress in the difficult field of biological research seems to start from those points where the situation is clear. The ever-present attempts of the obscurantist to befog the issue by over emphasizing what is not understood is a procedure too familiar to call for more than passing comment. Doncaster's book will therefore serve to give balance to the situation that is "developing normally."

There are few minor points in the book that call for comment. The author has, on the whole, most judiciously assigned special discoveries to their authors without overburdening the text with names. The omission of Stevens's name on page 63 in connection with the discovery of the XY chromosomes in relation to sex determination is an oversight, but some fuller mention might have been expected in connection with the history of these chromosomes when much less important matters receive their historical setting.

T. H. MORGAN

COLUMBIA UNIVERSITY

The Butterfly Guide: a Pocket Manual for the Ready Identification of the Commoner Species found in the United States and Canada. By W. J. HOLLAND, LL.D. $3\frac{1}{2} \times 5\frac{3}{4}$ inches; pp. 237; 295 figures in color. Doubleday, Page & Co. Cloth. $1.

Any guide book to the identification of 255 species of butterflies, that contains 295 finely colored figures, that costs only a dollar and actually does go into a vest pocket, may truthfully be called a great little book. This, in a few words, is a fair description of Dr. W. J. Holland's "Butterfly Guide." Apparently it is the first of its kind, and also the last word (and picture) in butterfly books for availability in the field and home.

The thirty-thousand-copy success of Dr. Holland's original "Butterfly Book" may justly be regarded as the inspiration for the present elegant booklet; and the author's point is well taken. This manual is built on the same general lines as Chester A. Reed's Pocket Bird Guide Series, and the "Birds of New Jersey." True enough, these volumes are none of them "reading books," and in the business of furnishing means to ends in identifying species they stick closely to their trails.

The purpose of this almost bewildering array of colored butterfly pictures is to promote identification of strange species, literally in a moment; and right well do they serve their purpose. Remembering as we do the breezy and rare freshness of the author's literary style, the only regret about this volume is that it does not and can not furnish room for unlimited Hollandesque gossip and disquisition on the more interesting species.

W. T. H.

SPECIAL ARTICLES

A NEW DISEASE OF GERMINATING WHEAT

WHILE examining some wheat fields on April 16 of this year it was noted that there was a

considerable unevenness of the stand, there being quite a proportion of very weak plants. It was known during the season of 1914 that these fields were infested by the wheat straw worm, *Isosoma grande* Riley, and it was thought that the weakened plants were due to infestations of this insect. Careful examination, however, did not reveal the presence of larvæ in the unthrifty plants. While making observations two weeks previous to this time it was noted that the emerged females of the wheat straw worm, *Isosoma grande,* were ovipositing in the wheat plants, and it was naturally our first thought, on examining the fields the second time, that the weakening of the plants was due to the attack of the larvæ. It may be said in passing that the wheat plants were largely volunteer, although some additional seed had been sown in the field.

A number of these plants were carefully examined in the laboratory and it was found that the attached wheat kernels were infected with a fungus which had apparently destroyed their contents at or near the time of germination. Pure cultures of the organism were made and it was found that the fruiting was typical in every respect excepting on nutrient agar cultures, or cultures which tended to become dry too readily.

A search of the literature indicates, in so far as I have been able to determine, that this disease has not been heretofore noted and that the organism has not been previously described. The fungus clearly belongs to the genus *Podosporiella.* We find only one other species under this genus, namely, *Podosporiella humilis* Ell. & Ev. The fungus is not truly parasitic, but seems to attack the wheat kernel about the time of germination, completely destroying the contents in very much the same way that the kernel is destroyed by smut. The result is that the wheat seedling, not getting the proper food supply in the early stages of growth, is permanently dwarfed and produces few stools. The crop yield is much reduced.

An extended description of the fungus and the characteristics of the disease will be given in the near future, at which time the fungus will be named as a new species of *Podosporiella.*

P. J. O'Gara

Occurrence of Thielavia basicola as a Root Parasite of Watermelons in the Salt Lake Valley, Utah

During the current season my attention was called to a serious trouble of watermelons, *Citrullus vulgaris* Schrad., in which all the plants in an entire field had been lost and even a second planting had largely died. Many of the plants came above the ground in an apparently healthy condition, but soon wilted or "damped off." Some that did not wilt had a chlorotic appearance and upon carefully removing them from the soil it was found that the lower part of the root system had been destroyed. These plants had developed many lateral roots above the point of injury. Examination showed that the roots were badly infected with the fungus *Thielavia basicola* (B. & Br.) Zoph. In going over the literature I find that Gilbert[1] gives a considerable list of hosts and the distribution of the fungus. This list does not include the watermelon and it is therefore apparent that the watermelon is a heretofore unreported host for this fungus. So far as the writer has been able to determine, *Thielavia basicola* has not been found in any part of the United States west of the Mississippi River; at least, it has not been found as an active parasite.

The fungus has been isolated in pure culture and has fruited characteristically, agreeing perfectly with the descriptions as given in the literature.

P. J. O'Gara

Occurrence of the Bacterial Disease of Sudan Grass in the Salt Lake Valley, Utah

Only very recently has Sudan grass, *Andropogon sorghum,* been introduced into Utah, and with it apparently has been introduced the bacterial disease. Very recently some specimens were brought to the laboratory for examination, where it was found that they were badly diseased. The elongated, red-brown blotches were extremely numerous and had caused the death of many of the leaves.

[1] Bulletin 158, Bureau of Plant Industry, U. S. Department of Agriculture, October 7, 1909.

Most of the lower leaves were entirely dead. On the under surface of the spots there was apparent the characteristic red crust or scabs. This crust consisted of dry bacterial ooze which had come from the interior of the blade. When sections of the younger spots were put in a droplet of water and placed under the microscope the bacteria could be seen oozing from the infected portions in enormous numbers. Pure cultures were readily obtained; some of the plates contained practically no other organism but the causative agent. Colonies on nutrient neutral agar formed rather slowly, being circular, white or pearly-white in appearance. On putting a platinum needle into a colony and lifting it, it was noted that the organisms adhered in such a way as to be stringy or sticky and could be drawn out to considerable length.

It was at first thought that the organism might be one previously described by Smith[1] as *Bacterium andropogoni,* but there seems to be little doubt that the disease is due to the broom-corn bacterial organism first studied by Dr. Burrill, namely, *Bacillus sorghi* Bur. This disease is one of the chief drawbacks to the culture of Sudan grass on the Gulf Coast, but whether it will be serious or not in the Salt Lake Valley remains to be seen. During the present season the month of May was very rainy and humid and this probably accounts for the rather serious infection of the plants. Under normal weather conditions it is quite possible that the disease will not prove a serious drawback to growing Sudan grass as a forage crop.

P. J. O'GARA

SALT LAKE CITY, UTAH,
June 28, 1915

THE PENDULUM KEY AND ITS USE FOR RECORDING THE BEATS OF A METRONOME

THE pendulum key is a short lever, pivoted at one end, and held vertically. It is so arranged that a slight lateral displacement of its lower end causes it to make an electrical contact. A platinum pin in this part of the lever strikes against a platinum plate fastened to the insulated piece from which the lever is suspended. This simple apparatus is an excellent arrangement for recording the beats of a pendulum or a metronome. The writer has found it easy to construct such an instrument by making a few additions to a key with a platinum contact made by the Harvard Apparatus Company, and used in physiological laboratories to make and break a current by hand. The sketch shows the key with the additions to hold it in a vertical position. The

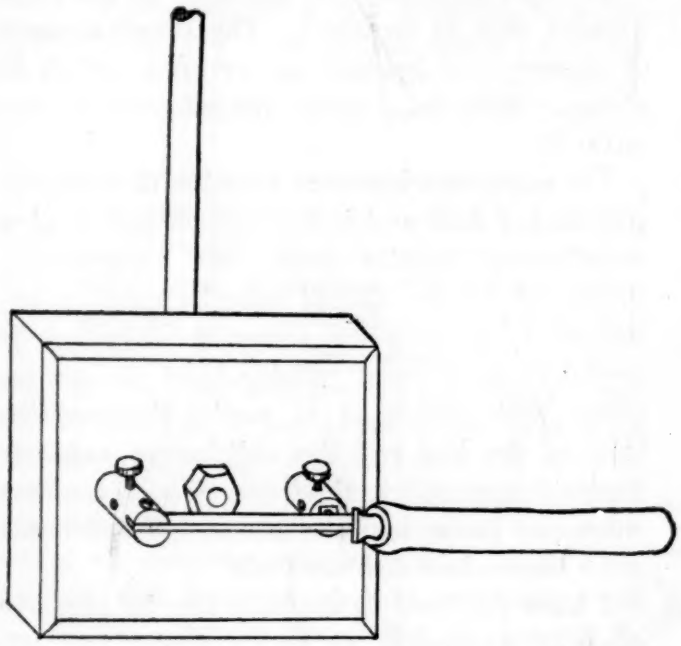

FIG. 1.

end of a short stud passes through a hole drilled in the middle of the slate base, and is held by a nut. A steel rod is screwed into the other end of the stud with its axis parallel to the plane of the base, and at right angles to the axis of the stud. The rod is held horizontally by a clamp fixed to the vertical rod of a tripod stand. By slightly rotating the key around the horizontal rod as an axis, the distance separating the platinum point from the plate against which it strikes can be varied to any desired extent. The lever can be lengthened by slipping one end of a short piece of rubber tubing over the handle, as is indicated in the figure. The tubing acts as a spring in breaking the shock of the impulse of the vibrating rod. The apparatus is placed in such a position that it receives a slight tap at the end

[1] "Bacteria in Relation to Plant Diseases," Vol. 2, 1911, Erwin F. Smith.

of the swing of the pendulum; the pin is brought in contact with the plate, closing an electric circuit, which actuates a time-marker writing upon the recording surface. As the lever rebounds, and does not make contact again until it has received another impulse, the electric closure is almost instantaneous.

The great advantage of this simple arrangement is that it does not involve any alteration in the apparatus with which it is used. A cork disc or ball slipped upon the end of the vibrating rod is the only addition to the metronome that is required. The disadvantages of a mercurial contact are avoided, which is always desirable, unless special reasons require it.

The apparatus has been tested with a recording tuning fork and it has been found to give satisfactory results with the metronome, which, of course, should only be used in experiments of moderate accuracy. The key is probably much more reliable than the metronome with which it is used. Comparative tests of the key and the well-known tambour device for recording the beats of a metronome were also made, and the key was found much more convenient and accurate.

I have ventured to describe this inexpensive piece of apparatus, as its simplicity and efficiency would seem to commend it to all those who employ the metronome in recording time.

Frederick W. Ellis

Monson, Mass.

SOCIETY OF AMERICAN BACTERIOLOGISTS

V

Industrial Bacteriology

Under the supervision of R. E. Buchanan

Problems in Soil Bacteriology: Jacob G. Lipman.

The student of soil bacteria, and of other soil microorganisms, is often struck by the fact that there is, apparently, localization of one or another of the species in certain spots. To what extent is this localization characteristic of fields, small areas in any one field, or soil particles of different mineralogical or other origin? We know, of course, that the water films surrounding the individual soil particles represent solutions of varying composition and concentration. But we have no knowledge, except of an indirect character, as to the very interesting differences which must exist as to the numbers and kinds of bacteria in the water film surrounding individual particles.

From other fields of bacteriology, we know that there may be associative action, and likewise antagonism, among species of soil bacteria. But we know practically nothing of these relations in the soil, nor how these relations may be modified by soil treatment or by climatic conditions.

It has always been my belief that the beneficial results ascribed to applications of commercial fertilizers or of other materials may be due as much to the action of such materials on soil microorganisms as to the action on the crops themselves. There is need now for the study of soil bacteriological problems from this point of view.

Another problem which is widely recognized of importance to soil fertility is the formation of so-called humus in the soil. Admitting that humus is the result of biological activities, it is for us to discover how the composition of the resulting product is affected, not only by vegetable and animal substances from which it is derived, but by the type of microorganisms concerned in its formation.

To the problems already mentioned, I might add the systematic study of temperature, moisture, aeration and pressure as factors in influencing bacteriological activities in the soil. It is possible also, that so-called stimulants, like manganese, copper, zinc, etc., may react on the activities of soil microorganisms. These problems should receive the attention of, preferably, a large number of workers. It may be added that these and other problems studied systematically will help to throw light on the production and modification of plant food in the soil and on the great industry of crop production.

The Solution Versus the Soil Method for the Bacteriological Examination of Soils: P. E. Brown.

From a careful study of the methods which have been employed for the bacteriological examination of soils, it is concluded that the "fresh soil" method is the most rational which has yet been devised. A recently proposed modification of the solution method, while eliminating some of the objections to the old method, is considered to possess many objectionable features, so many as to be of very questionable value for the interpreting of results from the fertility standpoint. It is urged that careful comparative tests be carried out, in order to settle definitely the question of which is the "best" method for the bacteriological examination of soils.

Relation of Lime to Production of Nitrates and Mineral Nitrogen: F. M. SCALES.

The lime requirement of an acid soil was determined by adding varying quantities of calcium carbonate to weighed portions of the soil, moistening and, after an hour, testing with litmus paper until a quantity was found that gave a neutral reaction. The lime requirement by the Veitch method was the same as the above. Fractions and multiples of this requirement were added to 100 gm. portions of soil which received in addition for one duplicate set ammonium sulphate and for another duplicate set alfalfa powder. They were moistened with 18 per cent. of distilled water and incubated for three weeks at 28° to 30° C. Determinations of nitrate and mineral nitrogen present in the samples showed that the nitrifying bacteria were most active in the presence of 50 per cent. of the calcium carbonate requirement and the ammonifying and nitrifying groups combined in the presence of 75 per cent. of the amount required according to the chemical determinations. In this particular soil an excess of calcium carbonate was markedly toxic for the nitrifying organisms and not stimulating for the ammonifiers. Some common crop plants are to be grown on this and other soils containing varying quantities of lime to determine what relation exists between the lime requirement for optimum nitrification and for ammonification and nitrification combined and that for the best growth of the plants.

A Soil Sampler for Soil Bacteriologists: H. A. NOYES.

The object of this sampler is to furnish a piece of apparatus which will sample the soil under one system of cultivation as well as under another. It also becomes the container for the soil after the sample is taken.

The sampler is a brass tube 11 inches long, with one end made into a cutting edge. This cutting edge is so made that the soil is not appreciably compacted when the sample is taken. The end having the cutting edge is furnished with a tight-fitting brass cap two inches in height. The open end, plugged with absorbent cotton, makes the sampler complete. The procedure in using this apparatus follows: Plug and cap as many samplers as you wish to take samples of soil; sterilize them in the hot air sterilizer and take them to the field. Remove a cap from a sampler, insert the driving head above the cotton plug and drive the sampler into the ground to the desired depth, pull it out, flame and return the cap and the sample is ready to take to the laboratory.

The sampler has the following properties which are important in bacteriological work: Easily sterilized; easily kept clean; easily manipulated; durable.

The Effect of Phosphates and Sulphates on Soil Bacteria: E. B. FRED.

The influence of inorganic fertilizers on the bacterial processes of the soil has not received much attention. For this reason a study of the effect of some of the pure salts of those elements which constitute an important part of commercial fertilizers was undertaken.

The aim was to determine, if possible, the influence of phosphates and sulphates upon the activities of soil bacteria and determine if the fertilizing effect of these substances could be explained in part by the promotion of bacterial action.

The following methods were employed:

Rate of ammonification in solution and in soil; this was conducted with pure and with mixed cultures of bacteria. Aside from this, determinations were made of the relation of the number of cells to the amount of nitrogen ammonified. To show this relation, plate counts were used. The nitrogen for ammonification was added to the solution in the form of peptone and to soil in the form of casein. The rate at which the nitrogen of these substances is converted into ammonia, was determined by distilling with magnesium oxide. The cultures were incubated at room temperature and at different intervals the amount of ammonia determined.

Monobasic potassium phosphate in peptone solution caused a great increase in the production of ammonia. This is noted with a pure-culture yellow ammonifier and with a suspension of soil bacteria. The gain was greatest at the end of the first two days.

Merck's precipitated calcium phosphate caused a slight increase in ammonification, but not nearly so large as the monobasic potassium phosphate.

Sulphates of calcium and potassium increased ammonification to a small extent.

The action of monobasic potassium phosphate was far greater than that of potassium sulphate. From this it seems that the potassium ion does not materially influence ammonification.

The results of plate counts show that monobasic potassium phosphate causes an enormous increase in multiplication of bacteria. This is followed by a rise in ammonia. The ammonia production, however, is not in proportion to the number of bac-

teria. This seems rather to be a result of increase in the number of cells than increase in individual cell activity.

All of the phosphates gave a large increase in the number of soil bacteria. There was only a slight increase from the sulphates.

The same relative effect of phosphates and sulphates was noted in the case of carbon-dioxide evolution.

From the results of this work, as a whole, the following conclusion may be drawn:

That possibly the increased crop production which results from the application of soluble phosphates is due in part to the promotion of bacterial activity.

The details will appear in a future publication.

The Effect of Green Manures on the Germination of Various Seed: E. B. Fred.

When green manures are turned under and the soil planted immediately, a decrease in germination may result.[6]

This problem was considered of sufficient importance to warrant a series of field and laboratory experiments in an endeavor to find some explanation for this phenomenon. The causes that might be offered to account for the harmful influence of green manures on seed germination are:

First, that the green manure not only causes a marked increase in number of bacteria, but also a change in the flora.

Second, that the great increase in number of bacteria results in a possible accumulation of some substance or substances, toxic to germination.

Third, that the rapid multiplication of microorganisms greatly increases their metabolism.

In order to gain some idea of the practical importance of this problem, a series of field tests was conducted. The results of this work show that when green clover or oat tissue is turned under and the land planted immediately, there is a distinct decrease in the rate of germination with cotton, soy bean and hemp seed. The cereals, corn and oats fail to show any injury from green manures. After twenty-five days the injurious factor seems to have disappeared entirely.

Under greenhouse conditions it has been found that small amounts—0.25 per cent.—of green manures are injurious to the germination of cotton seed. Larger amounts are more effective.

The addition of calcium carbonate to the green manure fails to prevent the injurious action.

The degree of retardation seems to vary somewhat with the soil type; in heavy soils green manures have their most marked effect; furthermore an increase in moisture causes a decrease in rate of germination.

When peptone and casein are added in the same nitrogen ratio as the green manure, no decrease in germination is noted. Soluble carbohydrates in amounts of 1 to 2 per cent. retarded the rate of germination, but did not cause the seed to decay as in the case of green manures.

Determinations of carbon dioxide and ammonia in green manure soils were made. Periodic analyses failed to show the presence of these in quantities great enough to account for the injury to seed germination.

A more complete report of this work will appear in bulletin form.

Standard Methods of Bacteriological Analysis of Milk: H. W. Conn.

Professor Conn gave an account of an extended series of cooperative experiments in four laboratories in New York upon the reliability of the bacteriological examination of milk, and as a result of the facts that were brought out by the cooperative tests, reported that the Committee on Standard Methods of the American Public Health Association had made the following changes in methods of milk analysis.

First, that beef extract (Liebig) be substituted for beef infusion in the making of agar media.

Second, that 1.2 per cent. dry agar or 1.5 per cent. ordinary moist agar be the amount used in standard medium.

Third, the acidity of standard medium shall be 1 per cent.

Fourth, all plates shall be incubated at 37½ degrees for 48 hours before counting.

Fifth, plates shall be counted with a magnifying power of 3½ diameters.

The Alkali-forming Bacteria Found in Milk: S. Henry Ayers and Philip Rupp.

The alkali-forming bacteria may be broadly defined as those which produce an alkaline reaction in milk within 7 days, due to the oxidation of salts of organic acids, which results in the formation of alkali carbonates. No visible sign of peptonization is produced.

Probably all the alkali-forming bacteria produce ammonia upon long inoculation, but the preliminary alkaline reaction is due to the production of alkali carbonates and not to ammonia. The presence of alkali carbonates in milk can be determined by the addition of casein dissolved in sodium phosphate.

[6] Hoffman, Exp. Sta. Bull. 228, 1913, p. 26.

Alkali-forming bacteria are very common in milk, but would rarely be noticed on litmus-lactose agar plates. They can be found by inoculating into tubes of litmus milk and observing the reaction after 7 to 14 days' incubation at 30° C.

The alkali-forming bacteria can obtain their nitrogen from meat juices, peptone, casein, gelatin, and many can also, with few exceptions, use nitrogen from inorganic salts, such as sodium ammonium phosphate and probably all ammonium salts; also from sodium nitrate and nitrite.

The best source of carbon seems to be the salts of organic acids.

Since various cultures ferment salts of different organic acids, we hope to be able to classify this group of bacteria on these fermentations. In sugar broths only an alkaline reaction is produced and consequently the sugar fermentation is of no value as a means of classification.

We believe the fermentation of salts of organic acids will be of great value in the classification of bacteria, particularly soil bacteria, which do not ferment sugars when present in broth.

Decomposition of Casein in Presence of Salt by Butter Flora: CHAS. W. BROWN.

The casein in butter during storage is slowly broken down into amino-acids and ammonia. Nitrogen, as amino-acids and ammonia, in percentage of the total nitrogen in unsalted butter (average of tubs from three creameries) was found to be 5.71 per cent. at first and 7.59 per cent. after 240 days' storage at 21° F.; in salted butter from the same three churnings and stored in the same storage was found 5.71 per cent. at first and 8.19 per cent. after 240 days. And again, in salted butter made from pasteurized and from unpasteurized cream, the percentage (average of 20 tubs) increased from 6.24 to 6.86 per cent. for the pasteurized and from 7.68 per cent. to 8.25 per cent. for the unpasteurized during storage at 0° F. for 428 days.

Pure cultures of twelve different bacteria isolated from storage butter when introduced separately into flasks of sterile separated milk and also into other flasks of the same milk to which was added 5 per cent. sterile salt and incubated at 20° C., caused a decomposition of the casein during 1, 3 and 7 days as follows: The nitrogen (per cent. of total milk) found as caseoses and caseones (average for 12 different bacteria) was 0.031 per cent., 0.037 per cent. and 0.054 per cent. in plain milk; and 0.030 per cent., 0.034 per cent. and 0.042 per cent. in milk with 5 per cent. salt; the nitrogen found as amino-acids and ammonia was 0.031 per cent., 0.042 per cent. and 0.076 per cent. for plain milk and 0.028 per cent., 0.035 per cent. and 0.041 per cent. for salted milk.

While the activities of butter flora in the decomposition of casein milk with or without salt can not be considered to parallel their action in butter, yet can we not assume safely that at least part of the casein decomposition in butter is due to the butter flora?

The Presence of Streptococci in the Milk of Normal Animals: J. M. SHERMAN AND E. G. HASTINGS.

In many public-health laboratories the routine examination of milk includes tests for streptococci. The supposed relation between udder streptococci and septic sore throat in man is the reason for making such examinations.

The examination of the milk from 88 individual animals in four herds demonstrated the presence of streptococci in 38.6 per cent. of the samples of milk. The animals were all free from udder trouble. The examination of the product of twelve herds demonstrated the presence of streptococci in the milk of ten of the twelve examined. In all of the above cases 1/100 c.c. of milk was used.

It would seem that the milk of healthy animals frequently contains streptococci at the time it is drawn from the udder, and that before much emphasis can be placed on the detection of these organisms in milk, methods by which harmful types can be differentiated from the harmless ones must be devised.

The milk of most of the herds examined was used chiefly for the feeding of children. No known cases of trouble have resulted.

The Refrigeration of a City's Milk Supply: CARLETON BATES.

This paper sets forth the plan of a milk campaign as conducted by the Bureau of Chemistry, U. S. Department of Agriculture. It further sets forth the results of the bacteriological examinations of a city's supply, the causes of the high bacteriological counts obtained, and means employed for remedying the causes.

The chief cause of the high bacteriological counts was due to non-refrigeration of milk in transit, the average temperature of the milk upon receipt in the city being about 65° F. This milk was en route from six to twelve hours.

After refrigeration had been provided by the railroads the milk, at the present time, is being received in the city at about 48° F.

Slimy and Ropy Milk: R. E. Buchanan and B. W. Hammer.

A study of slimy and ropy milk sent for examination to the Dairy Bacteriological Laboratories of Iowa State College has shown the following:

1. Cultures of organisms secured from slimy starters, apparently typical *Streptococcus lacticus* forms, sometimes showed marked capacity to produce ropiness when inoculated into sterile milk.

2. Associative action of organisms in some cases is responsible for ropiness.

3. *Bacterium* (*lactis*) *viscosum* is one common cause of slimy milk.

4. Certain peptonizing bacteria, as *Bact. peptogenes* produce a very slimy residuum after digestion of the casein.

5. *Bacterium bulgaricum* and certain related high acid organisms frequently produce marked viscosity in milk.

Sliminess in milk, therefore, is apparently due to different causes with different organisms.

Methods of control and prevention of slimy milk are discussed.

Descriptions of thirty-three species of bacteria that have been found associated with milk are given, and the literature reviewed.

Factors Influencing the Resistance of Lactic Acid Bacteria to Pasteurization: K. Peiser.

In milk and cream pasteurized at 63° C. (145° F.) for twenty minutes in a "Perfection" Pasteurizer (200 gal. capacity) were found a number of strains of the *Bact. lactis acidi* type whose thermal death-point in broth is below the pasteurization temperature.

The thermal death-point of a number of these strains was determined in bouillon (10° acid to phenolphtalein) and in boiled whole milk, separated milk and milk serum, with the result that the average thermal death-point is in whole milk 5° C., in separated milk 2.5° C., and in whey 0.5° C., higher than in bouillon. These results indicate that the protection given to the suspended lactic bacteria by the casein and coagulated albumen of separated milk raised their thermal death-point 2.5° C. and that the protein and fat of whole milk raises their thermal death-point 5° C. In this we see a reason why some bacteria whose thermal death-point is low are found in pasteurized milk.

Bacteria in Preserved Eggs: Maud Mason Obst.

Commercial and strictly fresh June eggs packed in solutions of 1:5, 1:10, 1:15, 1:20 parts commercial waterglass, and in saturated lime solutions were stored in laboratory, barn, cellar and at 34° F. Thermograph records were kept. Bacteriological and chemical examinations were made, also cooking experiments and parcel-post shipments.

Temperature of 80° F. in laboratory permitted rapid multiplication of bacteria in eggs.

Barn temperature varied from 10° F. to 87° F. Eggs froze in solutions, later some thawed without breaking and at end of experiment showed no effects attributable to freezing. Bacterial content was uniform and fairly low. Bacterial increase in commercial eggs in 1:10 waterglass was rapid, especially in albumen, during first two months of storage.

Eggs stored in cellar held a uniformly low bacterial content throughout experiment.

At 34° F. eggs showed exceptionally low counts.

Waterglass solutions contained practically no bacteria per c.c. after five months of storage. Average bacterial content of eggs in nearly every lime solution increased more rapidly than in waterglass, necessitating the discard of certain lime solutions early in experiment.

Curves were plotted showing increase of average bacterial content in relation to length of storage. Bacterial content of albumen in most cases remained lower or equal to that of yolks for 150 or 250 days of storage, then the former increased markedly and generally far exceeded that of the yolk.

From good eggs were isolated: *M. aurantiacus, B. prodigiosus, B. subtilis, B. pyocyaneus, B. fluorescens liquefaciens, B. termo, B. zopfi.* One decomposed egg contained *B. proteus* in large numbers.

Some Methods and Appliances Used in the Elementary Courses in Bacteriology: W. H. Wright and E. G. Hastings.

A description of the laboratory equipment used with large classes.

The Effect of Certain Organic Soil Constituents on the Fixation of Nitrogen by Azotobacter: Bruce Williams.

This paper reports a study on the effect of various organic compounds on the growth of Azotobacter. The compounds used were those likely to be constituents of the soil.

One liter Erlenmeyer flasks, to which were added 15 grams of pure sea sand, previously washed and burned, afforded an excellent surface upon which *Azotobacter* developed. To each of these flasks was added 100 c.c. of Ashby's media.

The flasks were sterilized under 15 pounds of steam pressure for 15 minutes. After this sterilization, the compounds were introduced into the flasks in desired concentrations and all flasks received equal inoculation of pure cultures of *Azotobacter* previously grown on Ashby's agar and suspended in sterile water. Two flasks were set up for each compound in every concentration and two control flasks receiving only inoculation were used to test the fixation power of the culture used. All flasks were incubated for 21 days, at the end of which time nitrogen determinations of the content of each flask were made by the Kjeldahl method.

In studying compounds which contain nitrogen four flasks instead of two were set up with each compound, two of the flasks receiving inoculation with *Azotobacter* and the remaining two used as controls for the nitrogen content of the compound—these latter flasks were kept in the incubator room during the period of incubation.

The concentrations employed were on the basis of p.p. Mil. or 0.025, 0.05, 0.1 and 0.2 gram per liter.

The results of the study indicate that fixation of nitrogen by *Azotobacter* is only slightly influenced by most of the compounds investigated.

Hydroquinone and salicylic aldehyde revealed the most toxic properties of any compounds studied.

Esculin, quinic acid and borneol afforded marked stimulation to the growth of the organism.

The effects of the compounds on *Azotobacter* are not, as a rule, in accord with what has been reported of their action on the higher plants. In concentrations which are fatal to certain higher plants, many of the compounds only slightly depressed fixation.

Such compounds as nicotine, picoline, guanidine and skatol exhibited toxic properties commensurate to those usually ascribed to these substances. Caffeine appeared to stimulate the growth of the organism.

Many of the nitrogenous compounds used which have been reported as beneficial to higher plants exercised a marked depression on fixation. It appears that the simpler compounds were more pronounced in this respect than were the more complex ones. It is suggested that this condition is not one of toxicity, but that the nitrogen of the compounds was utilized by *Azotobacter* in preference to that of the atmosphere. Urea, glycocoll, formamide and allantoin were especially active in depressing fixation.

Relation of Numbers of Streptococcus lacticus to Amount of Acid Formed in Milk and Cream: P. G. HEINEMANN.

Erlenmeyer flasks were filled with 250 c.c. each of milk and cream. Three flasks of each series were sterilized and then inoculated with a culture of *Str. lacticus* in litmus milk. Three flasks of raw milk and cream were also inoculated. Three flasks of each were left to sour spontaneously. The flasks were incubated at three different temperatures, 37°, 20° and 7°. Plates were prepared from the original milk or cream and the number of bacteria counted. The acidity was determined by titration with one twentieth normal sodium hydrate phenolphthalein as indicator. Every day for ten days the milk was titrated and counts made by plating. The determinations were made with the cream for eight days.

The following facts were observed:

1. The amount of acid formed during the souring process of milk or cream is not solely dependent upon the number of bacteria present of the *Str. lacticus* group. Temperature and the presence of other bacteria may influence the result.

2. In raw milk or cream or in raw milk or cream inoculated with cultures of *Str. lacticus* the number of bacteria increases to a given point and then decreases. The higher the temperature up to 37° C. the earlier is the maximum number reached.

3. Coagulation of milk or cream is not dependent solely upon a certain amount of acid or a certain number of bacteria.

4. After the decline in numbers the amount of acid continues to increase, probably due to enzyme action.

5. At 37° extraordinarily large amounts of acid may be formed, due probably to the presence of members of the group of lacto-bacilli.

The Variability of Two Strains of Streptococcus Lacticus: P. G. HEINEMANN.

The present investigation was conducted to determine the possibility of varying the fermentative power of *Str. lacticus* by animal passage. Two strains were isolated and inoculated into rabbits and guinea-pigs. The amount of acid produced by the original culture was determined by titration after three days' incubation at 37° C. After each passage the recovered organism was again inoculated into the solutions of test substances and the acid determined again. The amount of available free oxygen was regulated by filling nessler tubes with definite amounts of the

test solutions. The test substances used were dextrose, lactose, saccharose, raffinose, inulin, salicin and mannit.

The main conclusions reached by the work are:

1. The power to hemolyze human and goat's blood may be acquired to some extent by animal passage.

2. Animal passage develops and increases virulence of *Str. lacticus*.

3. Virulence develops more rapidly in rabbits than in guinea-pigs.

4. By animal passage the amount of acid produced in the original strain decreases progressively and fermentation of some of the substances is inhibited.

5. Raffinose and inulin, which were not fermented by the original strains, were fermented to a limited degree after animal passage.

6. Presence of free oxygen seems to favor the production of acid. Under anaerobic conditions less acid was produced than with free access of oxygen. Under anaerobic conditions fewer substances were fermented than under aerobic conditions.

Bacterial Infection of Fresh Eggs: Dorothy W. Caldwell.

This paper presented the results of a bacteriological study of fresh eggs carried on at the Agricultural Experiment Station of the Rhode Island State College. The results are, briefly, as follows:

1. Of 2,510 fresh eggs from 65 hens, examined by the indirect method, 8.8 per cent. showed infection in the yolk.

2. None of 111 whites examined showed infection, while the yolks of the same eggs gave a percentage of infection (4.5) less than the average for the series (8.8).

3. The percentages of infection obtained for individual hens per year varied between 2.8 and 15.0, the average being 8.0 per cent. per year. No hen laid sterile eggs during a whole year.

4. No correlation was observed between the percentage of infection for any individual and the degree of fecundity of that individual.

5. Approximately the same amount of infection was found among fertile eggs (6.9 per cent. infected out of 422 eggs examined) as among infertile (8.9 per cent. infected out of 315 eggs).

6. The infection of eggs in the degree made apparent by the present studies seemed to have no unfavorable effect upon their hatchability.

7. Practically no difference between the percentages of infection of eggs from pullets and from hens in their second laying year was found.

8. No definite seasonal variation was observed in the bacterial content of the eggs examined.

9. No definite conclusions can be drawn from these studies regarding the influence of temperature upon the detection of infection in fresh eggs.

10. From fifty-seven infected eggs out of 737 examined in one of the series, 37 bacterial types were isolated, among which were seven cocci, eleven motile rods, eighteen non-motile rods and one spirillum.

11. Control plates exposed under the hood in which the examinations were made yielded a variety of organisms, largely chromogens. This series, as a whole, did not resemble the series of egg organisms.

Regarding the source of infection, this study indicated that the penetration of the shell after the egg had been laid, or infections during the passage of the egg through the cloaca, or during fertilization or while the albumen or the shell were being deposited, are, to say the least, uncommon. It seems more likely that infection of fresh eggs is largely due to occasional chance infections with harmless organisms taking place within the ovary of the fowl.

A New Microscopic Test for Pasteurized Milk: W. D. Frost.

This test differs from a similar one described in 1911 by Frost and Ravenel, in the method of applying the stain, the nature of the stain and the principle involved. A few cubic centimeters of milk have mixed with them one fifth as much of a saturated aqueous solution of methylene blue. This colored milk is allowed to stand about 30 minutes; it is then centrifuged and the sediment spread on a glass slide. When dry it is ready for examination.

In raw milk the microscopic field is stained a uniform blue in which appear clear areas which are either fat globules or leucocytes. The polymorphonuclear cells are irregular in outline, about 12 mikrons in diameter and unstained or only slightly tinged. The sediment from milk heated to 60° C. or above presents a very different picture. The polymorphonuclear leucocytes are rounded up and shrunken so that they are only about 8 mikrons in diameter and the nuclei are deeply stained.

The method requires little more time than it does to make a fat determination and is apparently as simple and accurate as the laboratory diagnosis of diphtheria or rabies.

A. Parker Hitchens,
Secretary